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THE PARIS EXHIBITION—COMPLETION OF THE EIFFEL TOWER—M. EIFFEL RAISING THE NATIONAL FLAG.

COMPLETION OF THE EIFFEL TOWER.

THE erection of the 1,000 ft. tower has interested the Parisians to such an extent that the fete tendered Mr. Eiffel on the event of its completion has become an event of public importance. The tower was opened on March 31st last, when M. Tirard, premier, delivered an oration.

At half-past one o'clock Mr. Eiffel commenced the ascent of the tower, followed by about 200 invited guests, including Mr. Berger, the general director of the exposition, Mr. Contamin and the various heads of departments, Mr. Chautemps, president of the municipal council, etc. Three quarters of an hour later the company reached the fourth platform, at a height of 273 meters, an intermediate platform having been built for the use of the party between this point and the second halting place. The ascent is not yet finished. Another story brings them under the round cupola, divided into four apartments or divisions, three of which are for the use of distinguished visitors, and the fourth for Mr. Eiffel. The cupola is crowned with a lantern. An enormous hollow column, 60 centimeters in diameter, and with a ladder within, leads to the top. By means of this a dozen officials were admitted to this part of the tower—the top platform—simply a narrow circular gallery whence the eye wanders in wonderment to the four points of the compass.

The illustration shows the appearance of this platform at the moment that Mr. Eiffel unfurled the national flag. At this instant twenty-one guns were fired on the third platform. At the same time Mr. Contamin stepped up to the eminent engineer and congratulated him on the success of the enterprise. A few minutes later the party of officials reached the third platform, where toasts were drunk to Mr. Eiffel, and soon afterward the party arrived at the foot of the tower, where luncheon was being served to the workmen.—*Illustration.*

The great exhibition opens May 5 and closes Oct. 3.

FRENCH TORPEDO BOAT No. 110.

ONLY a short time ago torpedo boat No. 102 was sunk in the harbor of Ambiers, near Toulon, with a loss of ten men. Since then, a similar catastrophe has occurred in the loss of torpedo boat 110, on March 21, near Barfleur. Of course this has justly caused great excitement, and the Chamber of Deputies will have to take proper measures to correct these defects in the construction of naval guardians of the coast.

Admiral Krantz, the minister of the marine, spoke as follows before the tribune:

"On Thursday, March 21, at 11 A. M., four torpedo boats left Havre for Cherbourg. They were Nos. 110 and 111, length 118 ft., which had been fitted with new boilers, and No. 55, length 81 ft., and No. 71, length 111 ft. The weather at Havre was fair, but it was stormy at Cherbourg, and Admiral Lespes was apprehensive of trouble when he learned that the little fleet had put to sea under the command of Lieutenant Villiers-Mariame. The captain of No. 110 had received orders not to leave the harbor of Havre unless the weather was favorable. Nothing occurred in the early part of the voyage, but a heavy squall was met with near Barfleur. The weather changed. The sea became very heavy, and the little boats were washed from stem to stern. When the squall was over No. 111 and No. 71 could distinguish No. 55 putting back to Havre, which it reached after great difficulty about ten o'clock at night; but No. 110 could not be seen, and, what was still more alarming, one of the vessels discovered a large breaker out at sea, which was probably caused by a wreck. The weather did not admit of any attempt at rescue; besides, there was no one visible, and it was by no means certain that the wreck was No. 110, although this seemed probable. Nevertheless inspection boats and tug boats were sent out to search and the coasts were carefully watched, but no wreckage reached the beach." Such is the official report.

No. 111 reached Cherbourg considerably damaged. Nos. 55 and 71, being smaller, were uninjured.

Who is to blame for all this? The minister of marine states that every one, the engineers, officers, marine, the administration, in short, that every one could make mistakes, and perhaps even themselves. Nevertheless the Chamber of Deputies decided that such changes should be made in the construction of the vessels that the lives should be preserved of those brave officers and men who do not know what danger is, and simply, without question, do their duty.—*Le Monde Illustre.*

THE MANUFACTURE OF PARAFFIN OIL.*

By D. R. STEUART, F.I.C., F.C.S.

THERE have been several papers on shale distillation and paraffin extraction read before the Society of Chemical Industry, but there has been no description given of the general operations of refining. In this paper I mean to give a short sketch of the whole paraffin oil manufacture as carried on at Broxburn, and in such a manner that outsiders, who at present know nothing of the subject, may get a general notion of all our methods of working. Many members of the society visited the works last year, but only a small proportion, I think, could have heard the explanations given. I shall have to go over ground familiar to any one that has taken any interest in the manufacture, and describe apparatus and operations that have been described elsewhere, but in publications difficult to get at.

On the table I show samples of the bituminous shale from which we get all our products, and of the products themselves: sulphate of ammonia, naphthas, burning oils, lubricating oils, solid paraffins, gas oils, still coke. The naphthas are 0·730 and 0·740 sp. gr.—used as solvents, for burning in special lamps and for paraffin refining; the burning oils are 0·800 to 0·840, and the lubricating oils 0·865 to 0·895. There is soft paraffin, melting point 100° F., used largely for dipping the sticks of matches and for hand lamps, and paraffin wax, semi-refined and refined, with melting points from 118° F. to 130° F., used for candle making. Solid paraffin and paraffin oil are now much used for washing clothes.

The shale field, which since 1877 has been worked by the Broxburn Oil Company, Limited, was taken on lease by Mr. Robert Bell in 1858, and in 1861 the distillation of the shale was in full operation. This was the beginning of the shale industry in Scotland. For the ten years previous to this, during which the paraffin oil manufacture had existed, the Boghead or Torbanehill coal, was the mineral used. This mineral gave 120 gallons of crude oil per ton. In 1862 it ceased to be worked for paraffin oil and it was soon after completely exhausted.

The shale we work at Broxburn at present is what is known as the Broxburn seam. Its position geologically is in the lower carboniferous series, 140 fathoms or so above the Camps or Burdiehouse limestone (see *Journ.*, VI., 128). There are seven other seams at least worked in the Lothian shale field, and these lie some above and some below the Broxburn seam, and within 300 fathoms of it. They vary in thickness and quality in different districts, what is thick and rich in one district being poor or thin in another. The shales differ in character also, the Pumpherston seams, for instance, being poor in oil but rich in ammonia. The Broxburn seam averages in the Broxburn Oil Company's field about 5 feet in thickness, part of this thickness giving over 40 gallons per ton and part little over 20 gallons. It is worked in the same manner as coal. Where the retorts are placed there is none of this seam below, but it comes to the surface all round quite close to the works, and can be worked open cast. It gradually gets deeper, and mines are sent out radiating in all directions to get the shale at hand. The shale from these is brought by wire rope arrangement in the mine hutches direct to the breaker, and the platforms for the hutch railways are a conspicuous feature in the first glance at the crude oil works. Pits are sunk in various directions a mile or two further out to get the deeper shale, which is brought to the breaker by railway. The shale all passes through the breaking machines, where between two toothed iron rollers it is broken into pieces the size of a fist. The shale falls from the breakers into hutches, which are taken by endless rope to the top of the retorts, and tipped directly in. Manual labor is done away with as much as possible.

The first chemical process of the works is the destructive distillation of the shale. The shale contains by analysis: carbon, 20 per cent.; hydrogen, 3 per cent.; nitrogen, 0·7 per cent.; sulphur, 1·5 per cent. The compounds of these elements are no doubt of vegetable or animal origin. On treating pounded shale with shale naphtha or ether, the solvents remain quite clear and colorless, and dissolve nothing, proving that our products do not exist in the shale as such, but are all created by the destructive distillation. About 1,000 tons of shale are distilled at Broxburn per day, and the products are:

Volatile.	Per cent.
Crude oil	12
Ammonia water	8
Permanent gas	4
	— 24
Non-volatile (spent shale).	
Combustible	9
Ash	67
	— 76
	100

To get a good quality of crude oil the temperature of distillation must be low, a red heat invisible in daylight and faintly visible in the dark. In general, a higher temperature produces a heavier, darker crude oil, giving more loss in refining, and less solid paraffin, but a higher yield of ammonia. The only retort now used at Broxburn is Henderson's patent retort. Of these there are 784, arranged in 16 benches. It is a vertical retort of cast iron, 1¼ in. thick, 15 ft. long, and of oval section 2½ ft. by 1 ft. The charge is 18 cwt., and it lies in the retort for 16 hours, distilling off the crude oil, ammonia water, and permanent gas. After this the spent shale, which still contains 12 per cent. of combustible matter, is dropped hot into the furnace below to act as fuel. Four retorts are built into one oven and are connected with the one furnace, one retort being dropped every four hours to keep up the succession of heat. The products of combustion are taken off at the bottom of the oven instead of the top, so as to produce the gentle draught required by the weak fuel. The permanent gas of the distillation is also burned as fuel. It used to be passed among the spent shale, but it is now burned in the bottom of the retort oven. Steam, superheated by passing through pipes in the oven, is passed into the top of the retort. It introduces the right temperature throughout the mass of the shale, and sweeps the distillation products rapidly out of the heat, where they would undergo decomposition. No coals are required for fuel, except to make a start after a stoppage. The products pass off from the bottom of the retort to the main and condensers. The condensers are a series of vertical iron pipes cooled by the atmosphere. The crude oil and ammonia water flow from the condensers through a small vessel called a separator, where they at once part from each other by gravitation, the oil being the lighter, and pass into their several tanks. The non-condensable gases are drawn up through large towers, where they are washed, by water trickling down, from any remaining ammonia. Some naphtha is here condensed by the cold of the water, but several gallons of light naphtha per ton of shale, which we used to catch in blue oil, are at present allowed to go with the permanent gas. There are fans to suck the products from the retorts. After the spent shale has been burned, the bottom of the furnace folds down so as to drop the ash into an iron hutch below. The hutch with its glowing contents is run into the pond for dirty water, which it causes to evaporate, and after being quenched the residuum is taken by endless rope to the refuse bing.

This retort gives, without skilled labor or particular attention, the right temperature for making good crude oil. The mild temperature prevents the yield of ammonia from being nearly so high as it is with some high-temperature retorts; but against this defect we have the long life of the retort, the good quality of the crude oil, and the economy as to fuel. The temperature of the oven is from 900° to over 1,000° F.; the steam as it goes into the retort, 630° F.; the shale inside the retort, 1½ ft. from top, three hours after charging, 630° F., 16 hours after charging, 780° F.; and the products of distillation at the exit pipe, 500° to 600° F.

It has often been suggested that reagents should be put into the retorts in order to get an increase of ammonia and a purer crude oil; but as far as my experience goes with this particular retort, neither lime nor soda has any appreciable effect of any kind, and the dust, when the spent shale was dropped, made it very painful for the men. Solid reagents in the quantities that can be given on the large scale do not seem to come into sufficient molecular contact with the vapors. Gas reagents may be considered a little more promising. Mr. Tervet (*Journ.*, II., 445) got a greatly increased yield of ammonia by passing hydrogen into retorts at the end of the distillation. Mr. Irvine, by passing ammonia through retorts when the shale contained sulphur, got an increase in the solid paraffin. And Mr. Beilby got an increase of paraffin by passing air into his retort (*Journ.*, III., 220). In the Young and Beilby retort the large yield of ammonia from the hot part in passing through the part of the retort distilling the oil may help to give a higher yield of paraffin than would otherwise be the case at such a temperature; but how can air increase the paraffin? Can any of the oxygen reach the distilling oil so as to act on the sulphur or otherwise, or does the air act only by modifying the temperature?

A gas reagent used in all retorts is steam. It acts mechanically in protecting the ammonia and the complex hydrocarbon molecules from decomposition. At 1,000° C., according to Ramsay and Young, ammonia in presence of iron is nearly completely decomposed into its elements; but where steam is used in excess, as in the Grouven method of analysis, all the nitrogen of organic compounds is at this temperature converted into ammonia, sulphur appears as sulphureted hydrogen, the oxygen combines with the carbon, while some hydrogen is left free. Steam at such a high temperature is a powerful chemical reagent. At the comparatively low temperature of the Henderson retort its action is very gentle, but it does act on the carbon to some extent to give nascent hydrogen to help to build up ammonia and solid paraffin, and the yield of these is a good deal more than in the old vertical retorts worked at the same temperature.

Of the 1·5 per cent. sulphur in the shale, more than 1·4 per cent. remains with the spent shale when dropped from the retort, and 1 per cent. even after burning; 0·025 per cent. goes to the permanent gas, 0·028 per cent. to the crude oil, and 0·02 to the ammonia water.

The nitrogen in the shale is equal to 70 to 90 lb. of sulphate of ammonia per ton of shale. Of this we get no more than one-fourth, while nearly one-half remains with the spent shale. Mr. Beilby has studied the nitrogen very carefully in connection with his retort products. (*Journ.*, III., 216.)

I got samples of crude oil taken from a retort every two hours, and tested specific gravity, setting point, loss with oil of vitriol, and loss with caustic soda. I found there was a gradual increase throughout in the specific gravity—from 863 to 888—and in the setting point—from 78° to 87° F. The loss with oil of vitriol gradually increased from 5½ per cent. at the beginning to 10 per cent. in the middle, and then fell to 5½ per cent. at the end. The loss with soda gradually increased from 2·6 per cent. to 3·8 per cent. The quantity of oil coming for some hours at the end was comparatively small.

Each ton of shale produces in the works on an average 30 gallons of crude oil, 65 gallons of ammonia water, and nearly 2,000 cubic feet of permanent gas.

The permanent gas is sufficient in quantity to light up the crude oil works, fire the retorts along with the spent shale, and have a considerable quantity left to help to raise steam.

The crude oil is of a dark green color. It is semi-solid at ordinary temperatures, quite fluid when gently warmed, but setting or ceasing to flow at 80°, from the amount of solid paraffin in it. Our crude oil averages about 0·865 in specific gravity. The crude oil of other works is sometimes 0·880 or 0·890 sp. gr. Our own shale distilled in the gas works at a red heat gave about 4,500 cubic feet of rich gas; nine gallons of crude oil of 0·952 sp. gr., which was a thin mobile liquid, containing little solid paraffine, and 8½ lb. of sulphate of ammonia per ton. The small yield of ammonia shows the influence of the high temperature in the absence of steam.

The ammonia water averages 2½° Tw., or sp. gr. 1·012. It contains ammonia equal 0·277 lb. of sulphate per gallon, principally in the form of carbonate. There is a small proportion of sulphur present as sulphide, and traces of sulphite, sulphate, and thiosulphate; but no cyanide or sulphocyanate. The amount of fixed ammonia is small, and no lime is used in the distillation. The still we use is a column still 22 ft. long, 5 ft. in diameter, and with 14 trays. The three upper trays are open-topped annular channels; and the rest consists of an annular outer part, a disk-shaped central part, and a space between covered by an inverted piece which dips to the bottom of both the annular part and the disk part. The inverted piece is notched below, up to within half an inch of the surface of the liquid in the tray. The ammonia water enters at the top, flows from tray to tray, and is discharged at the bottom exhausted. Steam of 25 to 30 lb. pressure, introduced by a 2 in. pipe at the bottom of the column, rises through the space surrounding the disk, and passes through the notches of the inverted piece to bubble through the water of each tray. The system is a continuous one, and 35,000 gallons are passed through each still per day. It is found to be very economical in regard to labor and fuel, as well as in first cost of plant.

The volatile ammonia compounds are passed from the still into weak sulphuric acid. This acid has been used for refining the oil, and has been separated from the bulk of the tar with which it was combined. There are still strong bases in solution, and these are precipitated by the ammonia in the saturator boxes in the form of a black tar which rises to the surface. After separating the tar, the solution of sulphate of ammonia is evaporated down in vessels heated by coils of steam pipes. The salt as it separates falls to the bottom end, is fished out with a perforated ladle, and put up to drain and dry. In several days it is ready to be passed through a crushing mill into bags for the market. Neutral sulphate of ammonia on boiling gives off a little free ammonia, so the evaporating solution is always slightly acid in reaction. The temperature of the boiling solution rises to 225° F. Our sulphate is of

* From the *Journal of the Society of Chemical Industry.*

a gray color from a trace of impurities got from the acid. When the ammonia gases are passed into oil of vitriol, white sulphate of ammonia separates out and is fished direct from the saturator box. It was at Broxburn that ammonia was first made from the ammonia water of this manufacture, viz., by Mr. Robert Bell in 1864.

The operations of the oil refinery are (1) distillations; (2) treatments with chemicals; (3) cooling and pressing the heavy oil containing paraffin so as to separate the solid from the liquid.

The oil all gets several distillations—for purification and for thorough fractionation of the different gravities of oil from each other. Between the distillations the oil is treated first with oil of vitriol, and, after settling, with caustic soda, and these separate all the compounds present except the products which we want. The old method of refining was, first, to distill to dryness in cast iron round-bottomed stills. Naphtha came over first below the temperature of boiling water, the temperature gradually rose, distilling light oils, intermediate oils, heavy oils and paraffin, until at the end, when still grease came over, the bottom of the still was of a red heat. After cooling, the coke left in the still, 1½ ft. thick or more, was cleaned out and the still was recharged. In some works the distillation was stopped short of coking, and the residue run out as pitch. After chemical treatment the oil was redistilled from stills of malleable iron, which from their shape are called boiler stills. When the distillation was fairly started, oil was run into the boiler still which kept distilling lighter oil only, until the heavy oil accumulated in the still, when the feed of oil was stopped and the heavy oil distilled off until there were only a few inches left. The firing was stopped, and the residue, after cooling a little, was run into a coking still (similar to crude stills) to be distilled to dryness. The boiler still was cooled, cleaned out, and recharged. The stills in this old system were all half their time cooling down, cleaning, and heating up. Steam was used in all the distillations, and had to be constantly altered as the distillation proceeded, and its manipulation often caused the still to froth over into the distillate and pollute it.

The present system of distillation is a continuous one, in which wagon-shaped stills are connected together into a series. Let us take the crude oil as an example. The wagon-shaped stills are charged up at first (2,000 gallons) and the fires lighted, and when distillation is commenced the cocks on the connecting pipes are opened, and the feed oil is run into the central still, I. The feed is always heated on its way to the first still by the vapors of the distilling oil in a feed-heater of special safe construction shown in the drawings. Still I receives the oil at the front, and the oil travels along the length of the still, distilling off "green naphtha" of 0·753 sp. gr., which is condensed by an ordinary worm tank condenser. The oil not distilled in passing through still I is conveyed by pipes which proceed from the back of this still to the back of stills II, and along the length of stills II till they empty themselves at the front. The oil travels along these boilers, distilling off a higher boiling fraction, 0·835 sp. gr., and from the back of II a pipe conveys the concentrated oil to the residue or coking still, III. The hot oil is run into an empty coking still, which for safety has been previously fired up gently, and the air thoroughly blown out with steam. One of these stills remains connected in the series for eight hours, when it is disconnected and distilled to dryness by itself, and another coking still takes its place. The distillate from these stills runs from 0·860 sp. gr. to 0·965. This set of stills can put through 35,000 gallons of crude oil per day.

Other examples have three ordinary boiler stills connected with each other and with residue stills. The old form of boiler stills can be cheaply connected in this way. This kind is used at Broxburn for all the after distillations except that of the lubricating oil. The charge for each still is 3,000 gallons.

Steam, more or less, is passed into all the boiler stills. There is considerable power of adjustment as to gravities of distillate by the cocks on the connecting pipes, the firing and the steam, and once adjusted they require little altering. There is no danger of frothing over. Stills connected in this way can put through twice as much oil as when worked each by itself on the old plan. There is great saving in labor and fuel, as the boiler stills never require to be stopped or cleaned, and so do not require to be cooled down and heated up. There is also less cost in maintenance, as they are not subjected to the contractions and expansions of the cooling down and heating up of the old system. The fractionation is also more perfect and the distillates purer, giving less loss in refining, as the oils are always distilling from comparatively pure oils, and not, as in the old system, from concentrating impurities. For the last nine months our total loss in refining the crude oil has been only 26·7 per cent.

The stills have behind them a row of charging tanks raised above the level of the stills, so that the feed oil runs from them to the stills, by gravitation. Behind the charging tanks there is a row of tanks on the ground to receive the various distillates. Where it is possible the stills are arranged so that the products of combustion from one still requiring a high heat are used for firing another still requiring less heat.

When the crude oil is pumped from the retorts to the refinery, it first gets a distillation, and by it is fractionated into green naphtha and green oil with gravities of 753 and 858, the distillates of stills II and III being run together in our present way of working. The naphtha gets a treatment with chemicals and a distillation, and is then ready for the market. The green oil is treated and distilled, fractionating into light oils and heavy oils containing solid paraffin. The light oils require one or two treatments and distillations to make them ready for the finishing treatment. The heavy oil and paraffin after cooling and separation of the solid paraffin is called blue oil. With a treatment and distillation it is ready for its final treatment as lubricating oil. This is an outline of the course of the bulk of the oil, but at each distillation there is fractionation, and some small fractions get a greater number of distillations than I have mentioned to bring them to right gravities for the market, or to fit them for special purposes.

The treatments take place in boiler-shaped tanks of malleable iron with paddle-shaped stirrers. Air stirring, which, I believe, is most commonly used in other works, is little used in ours. As we use only the strongest sulphuric acid, and have the mineral oil thoroughly settled from water, the iron is little acted on by the acid. The acid is stirred in the oil for an hour or so, and the mixture allowed to stand until the acid combined with the basic impurities goes to the bottom as a black tar, and is run off below. The oil flows by gravitation into another stirrer, where it gets caustic soda of 60° Tw. strength (sp. gr. 1·300), and this precipitates the acid impurities, which settle to the bottom, and are run off also.

As to the quantity of acid used, we aim at giving at each stage as much acid as we can without acting on the olefines. With the proper quantity of acid given the soda tar is black, but if too much acid has been used the soda tar is a voluminous white blubbery mass. For the green oil or oil once distilled we use the vitriol tar of the final treatments to some extent, but the total treatment is equivalent to about 3 per cent. by volume of oil of vitriol. The intermediate treatments are small, but the finishing treatments are larger, and with them we designedly use acid sufficient to act slightly on the hydrocarbons, and get a white blubbery tar with soda. We have to give sufficient acid to produce the right color in the finished product, and when we give the right quantity we generally get the soda tar to settle out clear at once.

It is best to have the oil well refined before the final treatment, so as not to require to give much acid at this stage, for if much acid is required, although the oil may get quite bright after the soda, soda compounds are persistently retained in solution by the oil. These compounds should be easily washed out, and this should be thoroughly done. A finishing treatment, properly conducted, always lowers the gravity of the oil, but a badly conducted one sometimes increases the gravity, from the soda compounds retained. For the finishing treatment weak caustic has to be used, or soda carbonate solution. After the washings the finished oils are put into shallow tanks, exposed to light for 24 hours, to get bright. A thing that happens sometimes, and has to be guarded against, is that, when the soda is added, a whole tank of oil may set into a solid jelly. This takes place as readily with light oil as with heavy. It would happen unexpectedly, without any apparent cause. It was some time before we discovered that if the oil was brought very slowly to the neutral point after the acid, it was sure to set. Five per cent. of soap dissolved in warm burning oil causes it to set into a jelly on cooling, so that soap can be easily made to take up and retain paraffin oil; but, with the soda compound we have to deal with, less than 0·5 per cent. is sufficient to cause the oil to set. The amount of acidity left in burning oil on settling from oil of vitriol of a finishing treatment is equal to 0·1 per cent. of SO_4. The temperature of treatment with acid for the first treatments, particularly when much paraffin is present, may be over 100° F., but the finishing treatments are best at 60° F. or below it.

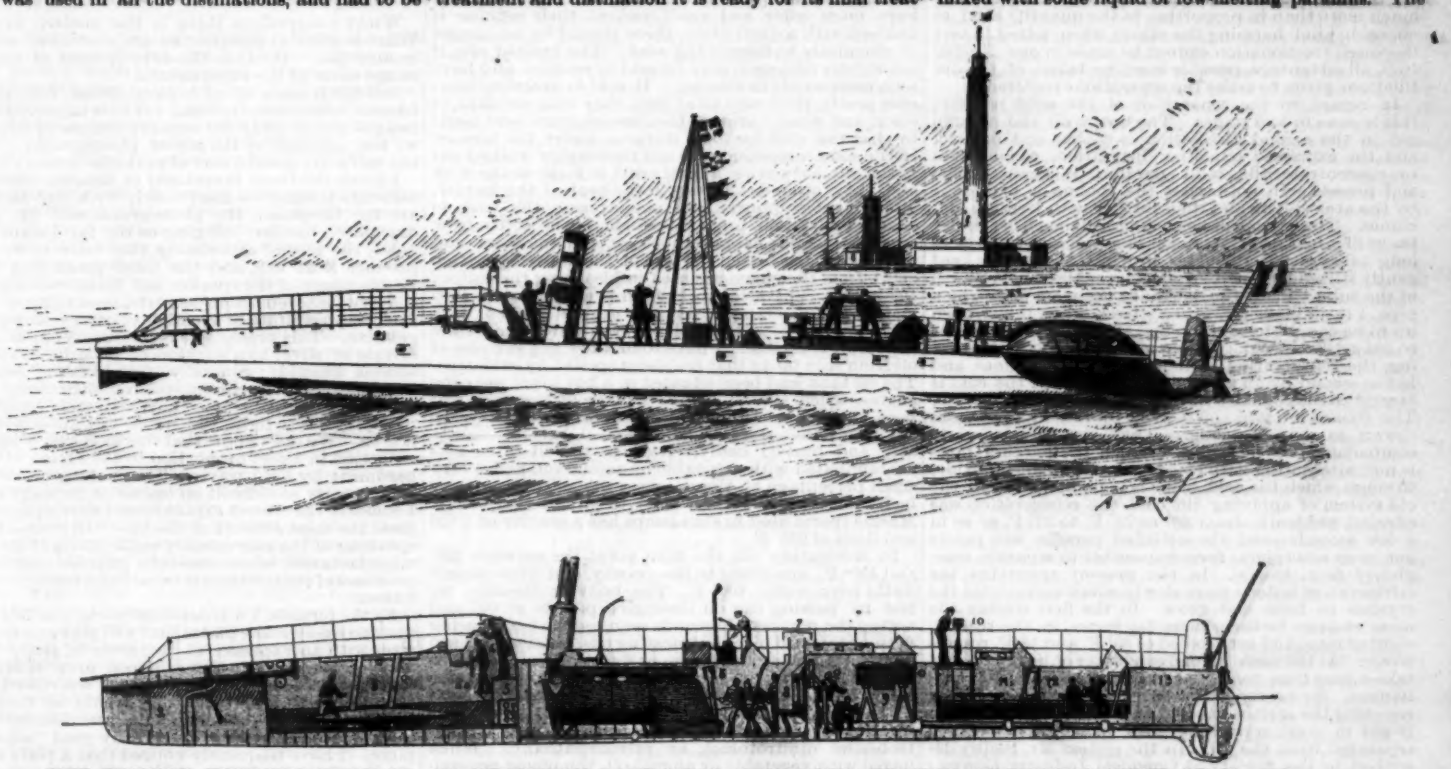
The vitriol tar is washed from sulphuric acid by agitation with hot water, and burned. It is blown into spray by a jet of steam, and the spray burns as if it were gas, giving a very high temperature, and producing little smoke. The acidity of the chimneys is practically no higher than when coals alone are burned.

Our products all belong to the two series of hydrocarbons—paraffins and olefines. Both series are represented in the permanent gas, naphtha, and burning oils, but when we come to the higher members of the series, the paraffins appear as solid paraffin and the olefines as viscous liquids. Lubricating oil consists of olefines mixed with some liquid or low-melting paraffins. The impurities which we have to get quit of in refining have been little investigated.

They are tarry substances that cannot be got to give crystalline compounds, and, on distilling, a large proportion is decomposed with the formation of olefines and a little paraffin. There is a proportion of strong bases that have been partly investigated, belonging to the leucoline and the picoline series. A portion of the impurities are ready to unite with either caustic soda or sulphuric acid, and are reckoned as partly belonging to the phenol group. Except these, there are no compounds of the benzene or anthracene type. The crude oil of our retorts requires a destructive distillation to begin.

If refining were started by chemical treatment, the loss would be increased, as substances are extracted which would by distillation be converted into products. Solid paraffin cannot be got from crude oil by cooling and pressing, and the paraffin exists partly in an amorphous form, and partly compounds are present which require distillation to convert them into paraffin. The paraffin, as determined in crude oil by Engler and Boehm's method, is about 8 per cent., while in the works we get fully half as much again. Zaloziecki proved that the oxidized compounds present in petroleum were converted by distillation into paraffins, and no doubt the same is the case with crude oil.

If too much steam is passed into the crude stills, the oil distills over very little changed, and the steam has to be restrained so as to make the distillation truly a destructive one. All the distillations are to some extent destructive at the end, and too much steam is often hurtful with the heavy residues. Steam is used pretty largely for the purpose of preserving the viscosity of the heavy oil, but if overdone it only causes extra loss with the chemical treatments. When the breaking up is fairly started at the end of a distillation, volumes of permanent gas are given off which, according to Beilby and McArthur, are very rich in light-giving properties. These products of decomposition also tend to give



THE FRENCH TORPEDO BOAT No. 110, RECENTLY LOST NEAR BARFLEUR, FRANCE—SECTIONAL VIEW OF No. 110.

1. Torpedo tube. 2. Forecastle. 3. Torpedo room. 4. Pilot house and observation tower. 5. Storage batteries. 6. Boilers. 7. Ventilators. 8. Ventilator for forced air. 9. Engines. 10. Cable for taking bearings. 11. Captain's room. 12. Mess room. 13. Storage room.

lubricating oil a low flash point if not looked after. Even light-burning oils in distilling deposit a little coke on any solid body their vapors may come in contact with, but there is no decomposition to speak of except with the very heavy oils. From what I hear of the Americans cracking up the intermediate oils of their petroleum so as to reduce them in gravity to burning oils by manipulating the firing of the stills, I am sure that the oils they have to deal with are much more easily decomposed than ours, and Russian petroleum seems to be even more easily decomposed. Cracking up is not required by us nowadays, as there is a good market for intermediate oils of all gravities for gas making, burning in special lamps, etc.

Water, on being converted into steam, increases in volume nearly 1,700 times, while burning oil increases less than 200 times, heavy oil less than 100 times, in being converted into vapor. So in distilling heavy oil without steam a great deal of oil has to be converted into vapor before it mounts up to the point of exit from the still, and as the specific heat is low it is easily condensed to fall back into the still again. The steam blown into the still is useful in preventing condensation and in carrying the vapor over mechanically into the condensers. Two non-miscible liquids distilled together have the temperature of boiling very much lowered, sometimes a good deal below the boiling point of the lowest one; and oil distilling into vapor of water boils at a much lower temperature than when distilling into its own vapors. Some heavy residues we distill without steam, some with steam blown over the surface of the oil, and others with superheated steam blown into the bottom of the still. Solid caustic soda is put into the residue stills in the last distillation of all the heavier oils.

Steam is of great importance also for fractionation. Thorough fractionation is the great secret in oil refining. A little naphtha in burning oil lowers the flash point, of course; a little intermediate oil in it reduces the capillarity and harms the burning; intermediate oil present in lubricating oil decreases the viscosity much more than in proportion to the quantity, and so on, each kind harming the others when mixed. As a thorough fractionation cannot be made in one distillation, all advantage possible must be taken of the distillations given to make the separations required.

In regard to the separation of the solid paraffin, this is done in two stages. The heavy oil and paraffin got in the second distillation is cooled and pressed, and the expressed oil, after distillation, which helps to concentrate the solid paraffin, is again cooled and pressed. In both cases the cooling is first done by the atmosphere and then further with freezing machines. The atmospheric cooling is done either in flat tanks of a foot deep, arranged in a cool airy shed, or in long tubes of 3 ft. diameter, in which the mass is kept gently stirred by a paddle which goes the whole length of the tube. From the flat tanks the mass passes by a pipe, 1 ft. in diameter, into a stirrer, when it is broken up to be caught by a pump and thrown into the cooler to get artificial cold. In our works, for the first pressing, the refrigeration is done by ether machines, and in the second by an ammonia machine, and the cold is carried to the coolers by a brine of chloride of calcium. The coolers are tanks 10 ft. long by 6 ft. wide, of form shown in the drawing. They are divided into compartments 6 in. wide, in which the paraffin mass is put, alternating with compartments about 1 in. wide, through which the cold brine is kept flowing. In the old system of applying the cold, the refrigeration was effected suddenly—from 60° or 70° F. to 30° F. or so in a few seconds—and the solidified paraffin was partly got in an amorphous form impossible to separate completely from the oil. In the present apparatus the refrigeration is done more slowly, allowing time for the crystals to form and grow. In the first cooling the mass remains in the coolers 3½ hours, in the second, eight or nine, and gets cooled to 30° F. and 15°F. respectively. As the mass is a bad conductor of heat, it would take a long time to get cooled throughout if left motionless. So a scraper slowly revolves in it, constantly renewing the surface exposed to the cold. The paraffin is got in good crystalline form, in which it is easily separated from the oil. In the system Mr. Beilby described to the Society of Chemical Industry (*Journ.*, IV., 331), the cooling is done even more slowly, requiring at the same time a proportionately larger plant. A paddle stirrer at the bottom of the tank prepares the mass to be caught by a pump to be pressed through the filter presses. From the filter presses the oil runs with a setting point of 30° F. in the first cooling, and from 15° to 22° F. in the second, according to the gravity of the oil. The solid taken from the filter presses is put into cloths and pressed in plate presses with hydraulic ram at 30 cwt. and 20 cwt. per inch, giving hard scale and soft scale respectively. The slow method of cooling gives an extra quantity of solid paraffin and a lubricating oil improved in setting point and viscosity. Solid paraffin, when dissolved or melted, gives a thin, mobile liquid, deficient in viscosity. So any left in the oil, besides being a loss in itself, deteriorates the quality of the lubricating oil. Solid paraffin, when liquefied, is equal in gravity to an oil which at 60° F. is about 810 sp. gr. A heavy oil of 863 sp. gr. and setting at 42° F., after losing 10 per cent. of paraffin, has its specific gravity raised to 872, and its setting point reduced to 20° F.

All the apparatus shown in the drawings are inventions patented by Mr. Henderson, the works manager.

In refining the paraffin scale, the processes used are of three kinds: treatment with chemicals, sweating, and treatment with naphtha. If refining is wholly done with chemicals, the quantity of oil of vitriol required is large, the temperature has to be very high, the loss is great, and the refined paraffin not particularly good in color. We use comparatively small treatments with oil of vitriol, carefully avoiding a temperature above 140° F., and this we find helpful to further refining. The soda compounds have to be very carefully washed out, otherwise they make the paraffin greasy and amorphous and difficult to refine. In the sweating process, the scale is melted and run into shallow tins to cool. On cooling, the highest melting point paraffins will separate first, and act as centers around which layers of lower and lower melting point paraffins will crystallize, concentrating the coloring matters in the oil and the softest paraffins. On putting the cakes of paraffin into warm chambers, the low melting point paraffins melt, and, along with the oil, flow out from the crystals of hard paraffin, carrying the coloring matters with them in solution. Our best refined paraffin is made by washing with naphtha. The scale is melted, and 25 or 30 per cent. of shale naphtha is stirred into it, and the mixture run into square shallow tins, where it solidifies into cakes suitable for the plate presses. On pressing, the naphtha runs out, carrying all the coloring matter. The presses are enveloped in a thin metal hood, which is raised or lowered as required by a counterpoise weight, and this helps to prevent loss of naphtha by evaporation. Two naphtha treatments are required. The melted wax has steam blown through, to rid it of the small quantity of naphtha still retained, and after settling from water, the wax gets a small treatment with char from the prussiate works, is settled, and filtered through cloth and paper, and on cooling into blocks, is fit for the market or the candle maker. In our busy season we make 14 or 15 tons of paraffin candles a day. Zaloziecki has some very interesting experiments on char recorded in "Dingler's Journal." His char seems to have contained much more alkali salts than the char we get in this country, with the result that our char can be recovered by distillation to be as powerful as ever, while his could not. In his case the alkali salts fused around the particles of char in the retort when recovering, which rendered it comparatively useless until washed free of alkali. We find that if the char remains too long in contact with the wax, the wax begins to take back the coloring matter to some extent.

A few words before I close regarding the properties of oils. All kinds except naphtha should have a high flash point, and should be free from acidity and from soda compounds. Oils for ordinary lamps should all distill over under 300° C. (572° F). Lighthouse oil is a good example of what a burning oil should be; it distills all over under that temperature, and its flash is over 160° F. Our ordinary burning oils have a very small residue boiling above 572° F., and a flash point of about 30° above the government safety test of 73° F. Burning oils are sent into the market in two forms, namely, distilled oils and treated oils. Distilled oils have more color and smell, and as their refining is finished with a distillation, there should be no danger of chemicals to incrust the wick. The treated oils, if not rightly managed, may be acid in reaction and have soda compounds in solution. If acid in reaction, however pretty they may be at first, they soon get dark in color, and when burning, the free sulphuric acid tends to char the wick for some distance under the burner. If the soda compounds are not thoroughly washed out of the oils, they cause a hard crust to form on the wick and they often decompose in the heat of the burner, giving free sulphuric acid and causing the wick to char. Treated oil, properly refined, burns as free from crust or acidity as a distilled oil, and the color and smell are much better. Wicks of themselves are often acid—from the chemicals used in bleaching the cotton, I suppose. The acidity of an oil is easily tested with methyl-aniline-orange. It has to be shaken very thoroughly, and afterward settled quite clear, before the result is certain. I have sometimes got samples of oil from ship tanks that had been spoiled by oxidation. The oil tank had been situated in a hot place near the engine, when the oil, by the motion of the ship, had been kept shaken up with air, perhaps for months, with the result that the oil had darkened, become slightly acid, and burned badly. In ships the oil often gets contaminated with vegetable or animal oil, and a very small percentage of these causes the oil to burn very badly, or even in a short time to go out completely. Marine sperm used in ship lamps has a gravity of 0·880 and flash of 230° F

In lubricating oils the flash point lies between 320° and 400° F., according to the gravity, and little should distill over under 600° F. The body or viscosity we test by passing the oil through a pipette at 60°, and noting the number of seconds required, and comparing with a standard oil. Lubricating oils, besides the ordinary form having the bloom or fluorescence characteristic of mineral oils, are sold also as bloomless oils. Dark bloomless oils are produced by oxidation with nitric acid or air, and the pale bloomless oils are made by dissolving nitro compounds in them, such as dinitrobenzol dinitrotoluol, or nitronaphthalin. When mixed with vegetable or animal oil, the bloom generally shows itself pretty strongly again.

The paraffin oil manufacture is carried on in Scotland by 14 companies, nine of which do refining, and the production is about 60 million gallons of crude oil per annum from two million tons of shale, and the value of refined products about 1,500,000*l.* The capital invested is about 2,000,000*l.*, and the men employed about 10,000.

Mr. Steuart, in reply to the discussion, said that while the several gentlemen were speaking, many things occurred to him that he might speak about, but he did not know whether he could now recollect them all. As to the ammonia column still, if it should happen to choke, the obstruction was easily got at from the side doors and removed, so that there was very little trouble from this cause. The stills were cleaned out once a year. He had never noticed the whitening of the shale, attributed to the steam acting on the coke, although the spent shale was sometimes left in the retort some hours beyond the usual time in order to increase the yield of ammonia. Where it had happened he suspected that air had been sucked in. Mr. Hamilton was surprised at their increase in refined products during the year. He had not the figures with him, but there was no doubt that the amount of loss had been gradually reduced from year to year. Last year it would be about 27 per cent. if he remembered rightly. Regarding the cooler, Mr. Henderson originally intended that the paraffin should remain at rest during cooling, and that the stirrer should only be put on at the end to bring it down, but the difficulty was to get the stirrer to move. It was found that by sending it slowly round all the time a paraffin was got that separated quite readily from the oil, and gave an oil of sufficiently low setting point. Of course, the more rapidly the cooling took place, the more economically it could be carried out. In regard to the amount of steam used in residue stills he, of course, had gone into that pretty fully in his paper. But it was found, in the case of green oil residue, that if a great amount of steam was passed into the still the paraffin scale extracted from the distillate was green and difficult to refine, and that no extra loss was incurred by restricting the steam according to the circumstances of the case. It was also asked what was the minimum melting point of paraffin. It could be got in quantities about 80° F., but the difficulty was in finding a market for such very low melting points. A question had been asked as to whether there was any special treatment required to make the paraffin transparent, as was the case with some shown at the Glasgow Exhibition. Paraffin wax was no doubt in itself transparent, but in its ordinary state it appeared opaque from the substance existing in the form of small crystals, with free spaces between. When gently warmed paraffin became plastic, and when pressed in this state, the interspaces were done away with, and the mass became transparent, like glass. The interspaces being reduced by pressure was no doubt also the explanation of the increase in specific gravity which Mr. Tatlock had remarked.

Regarding Mr. Tatlock's remarks as to getting a constant luminosity, he had no doubt that paraffin could be made of such a homogeneous structure that it would be practicable to get a constant luminosity, but the matter of wick entered considerably into the question as to whether a constant light could be got from the candles or not, but he had not made any experiments on the subject.

Addendum.

I find that up till 1885 our total loss in refining was 30 per cent. on the crude oil, but much lower ever since. The cost of refining in 1879 was 1·38*d.* per gallon of crude oil. For the last nine months it has averaged 0·77*d.*

In regard to retorts, I neglected to state that the total fuel required for breaking the shale, raising steam, haulage, pumping, and everything else connected with the crude oil, was 1 cwt. coals per ton of shale.

We cannot put caustic soda into the boiler stills, but it can always be put into the residue stills, which are the last of each series.

THE MAKING OF DRY PLATE EMULSIONS.*

WHAT a marvelous thing is the modern dry plate! What wonderful possibilities are contained within its creamy film! Does not the development of the latent image savor of the supernatural?

Our life is made up of images—home, wife, children, friends, town, country—and yet how imperfect are the images stored away by memory compared with those we may produce by the aid of photography. May we not call a dry plate a sort of portable memory?

I think the three inventions of modern times which associate themselves most closely with our inner lives are the telephone, the phonograph, and the gelatine dry plate; the first bringing us the far-distant human voice, the second embalming that voice so we may reproduce it at will, and the third preserving for us a visible image of the speaker and his surroundings.

A dry plate emulsion consists essentially of a finely divided precipitate of bromide of silver suspended in gelatine. This precipitate is formed by the addition of nitrate of silver to a solution of gelatine containing a soluble bromide; generally of an alkaline metal, such as potassium, ammonium, sodium, or lithium.

It is needless to tell you how extremely sensitive this precipitate is to light, but we do not know as yet why it is so. We only know that the molecule is capable of reduction in direct proportion to the extent of its bombardment by light waves. The making of a dry plate is not nearly so difficult an operation as many suppose. I consider the correct exposure and development of the plate the most difficult of the two. Of course I am not speaking of the marvelously rapid plates of the regular manufacturers, whose carefully guarded secrets cover processes of considerable intricacy and nicety of manipulation.

By the formula I will demonstrate to you this evening you can easily make plates that will give excellent negatives with an exposure of two seconds, stop *f*–15 upon an open landscape in June. Good drop shutter work may also be done with them. I am convinced by long experiment that the emulsion should be made with a view to the work for which it is intended, and that it is an impossibility to make a really good "all-around" plate. I have frequently noticed that a plate that will give good results in the studio will be super-sensitive to diffused light on landscape work, causing distant objects to disappear in fog in order to get sufficient exposure in the foreground. I often hear it said that commercial plates all have chemical fog, that the shadows are not clear glass, and yet you will notice that any part of the plate that has not been exposed at all to light will be clear glass. What ordinarily appears like chemical fog is really the effect of diffused light upon the gelatine. This will frequently occur when the light has been too weak or the exposure too short for direct reflections from objects to impress themselves upon the plate. You may try an interesting experiment to prove this by opening the door of your dark room so that a little diffused white light may fall upon half of a dry plate, place in weak developer for a few minutes and fix. The exposed half will present a mealy, ground-glass appearance, without apparent reduction of silver, while the unexposed half of the plate will be clear glass. Therefore a good landscape plate must not be too sensitive to diffused light, but it should be highly sensitive to reflected light from different objects, and give a good rendering of color values. For general landscape work I prefer a pure bromide to an iodo-bromide emulsion.

For interiors and portraiture, a small percentage of iodide is indispensable, as it increases the color sensitiveness for yellow and red. Of course my remarks apply largely to the ammonia process of dry plate making. I have experimented with nearly all the published formulæ and find that the ammonia process is the most reliable and by far the simplest method which the amateur can attempt, although I find many who are in favor of the boiling process. There is much difference of opinion in regard to the use of iodide in emulsions. With pure bromide I find greater latitude of exposure. I have not found any gain in rapidity for landscape work to result from the addition of iodide in the ammonia process. If two plates are exposed in sections upon the same landscape, one pure bromide and the other iodo-bromide, they will be found to have correct time upon about the same section.

It will be noticed that over-exposure on the pure

* Lecture delivered by E. H. Lyon before the Boston Camera Club. Reported in *American Journal of Photography.*

bromide only increases density through a considerable range, while over-exposure on the iodo-bromide plates will quickly result in a flattening of the lights. I will explain what I mean by exposing the plate in sections. Mark a plate holder slide in sections of a half-inch, and number them; put a plate in and expose the whole of it for one second; push the slide in half an inch and expose another second, and so on, being careful not to jar the camera. On developing the plate it will be seen at once which gives the clearest and best result. The correct exposure for that particular plate or emulsion will be equal to the sum of the short exposures on that section.

The mixing and digesting of an emulsion are the simplest parts. The troubles are likely to come after this, and are caused principally by the improper application of new gelatine. We are likely to seal up the delicately poised molecule we have produced, rendering it insensitive and difficult to reduce. I will show you how to make an emulsion which, for simplicity and excellency of results in landscape work, I think is unsurpassed by any of the rapid commercial plates. It is made by the ammonia process. As there is a good reason for each step in the process, the directions given should be closely adhered to.

Here is the formula:

A

Distilled water	4 drachms.
Ammonium bromide	30 grains.
Nelson's "I" gelatine	5 grains.

B

Distilled water	4 drachms.
Nitrate of silver	45 grains.

Precipitate and redissolve with strong ammonia.

C

Hard gelatine (Heinrich's, of Winterthur)	30 grains.

This is the way I mix the emulsion. Place the hard gelatine "C" in sufficient distilled water to cover it, and set aside to swell for one hour. Now take a wide-mouthed eight ounce bottle, and put in four drachms of distilled water and thirty grains of bromide of ammonium. Let this dissolve, and then put in five grains of Nelson's "I" gelatine, taking care that the solution covers it. Set this "A" solution aside to swell for one-half hour.

To prepare solution "B" weigh forty grains of nitrate of silver, and dissolve in four drachms of distilled water; add drop by drop enough strong ammonia to just redissolve the dark brown precipitate of ammonia-nitrate of silver. If an excess of ammonia has been added, it should be allowed to pass off into the air before the solution is used. The solution should have only a faint odor of free ammonia. Now pour the excess of water from the hard gelatine "C," and dissolve in a covered beaker or tumbler at 120° F. in the water bath. Then place the bottle containing the bromized gelatine in the water bath and dissolve at 100° F. The bottle should be provided with two corks, in one of which a small glass syringe is fitted.

All operations up to this point may be conducted in white light. We must now work by the light of our ruby lamp, which may also serve to keep the water bath warm during digestion. Let the "A" and "C" solutions fall to the temperature of the room, and add "B" to "A," a little at a time, by means of the syringe, at the same time shaking the bottle. Now pour in "C," thoroughly mix and place the bottle, which has been closed by cork two, in the water bath at 100° F., to digest for one hour. It should be shaken frequently during that time to prevent the formation of a coarse precipitate. A thermometer should be the inseparable companion of the bottle in the ammonia process, and the temperature closely watched. A cheap thermometer without a case serves the purpose. The lower portion of it is wrapped with two thicknesses of flannel, and a strong rubber band fastens it to the outside of the bottle.

Do not allow the temperature to rise above 100° F. during the digestion. This is very important, for at high temperatures ammonia is destructive to soft gelatine, and it appears to convert hard gelatine into a leathery compound which retards or entirely prevents the subsequent ripening of the emulsion. We may allow ourselves plenty of non-actinic light while mixing and during digestion. After washing and during the coating, we should be more careful. The process of digestion may be watched by placing a drop of emulsion upon a piece of glass from time to time, and examining it by transmitted light. When first mixed it will have a salmon color, changing to blue, and finally to a blue green. It should now be poured into a shallow dish to cool. This will take about an hour. When firmly set, take it up with a silver spoon, and squeeze through a piece of embroidery canvas into a dish of cold water. A large yellow bowl is useful for this, as the shredded emulsion may be readily seen by ruby light as it settles to the bottom.

Keep the shreds in motion with a glass rod. Allow them to settle every few minutes, and pour off the water, replacing it by fresh. Repeat this operation seven or eight times during twenty minutes, and the washing will be quite complete.

Pour off as much as possible of the last wash water, and add one-half ounce of alcohol to each ounce of emulsion. Let the shreds stand in this dilute alcohol for some minutes, then pour the whole upon a piece of soft muslin, stretched across a deep dish or bowl, gather up the corners of the muslin, and squeeze the shreds as dry as possible. Now spread the muslin upon the bottom of the washing dish, and with a silver spoon gather up the shreds and place in a cup or glass and remelt at 100° F. Then set away in a cool place to ripen for from one to five days. The addition of alcohol to the last wash water accelerates the ripening process.

The ripening of an emulsion appears to cause an organic change in the structure of the gelatine, after digesting or cooking, which brings about a granular condition. I would describe it as a mealy condition. Herein lies the extreme sensitiveness of a gelatine emulsion. Out of perhaps twenty different ammonia formulæ, nothing is said about the method of making the first "A" solution further than to combine the ingredients, and yet I find it is very important; the resulting emulsion depends for its speed and excellence very much upon these first steps. For instance, if I heat the water and bromide to a high temperature, and then add the gelatine dry, it will dissolve in a few minutes, but the emulsion will not be so rapid as when it is allowed to swell slowly and then dissolved at a very low temperature. I consider 100 degrees as high as it is safe to go with a gelatine solution that contains alkaline bromide. I use a pair of cheap scales, which are sold at the stock houses; a piece of tissue paper should be used to weigh out each ingredient upon, throwing it away, so that there may be no contamination, and it is very necessary in dry plate making that everything shall be scrupulously clean.

In regard to distilled water: I have made a great many emulsions with the ordinary tap water, and I find that it answers the purpose very well, but still at certain times it will not give good results, although you may make a great many emulsions with it and not have any trouble. The ordinary tap water will sometimes contain iron and sodium, or there may be some trouble with the pipes, and if you make a poor emulsion it is very valuable to know what is responsible for the failure, and if the water is right to start with, of course you can look elsewhere for it. Snow or ice water would be preferable to tap water, but as you use such a small quantity any way—for every hundred grains of silver only two ounces—it is better to obtain distilled water and use that only. The washing of the emulsion may be done with tap water.

Now, as I said before, temperature is a very important thing, and if the formula says 100 degrees, do not allow it to be as high as 110, for gelatine has been well called an unruly beast.

In order to keep the emulsion at a uniform temperature while digesting, use a small chemical stand provided with movable rings. The water bath may rest upon one of these, and, with a little practice, you will know about how far above the lamp it should stand in order to keep the water at 100 degrees. Of course the temperature of the room has something to do with that.

I would recommend the amateur dry plate maker to make small batches. Forty-five grains of silver will coat about a dozen 5 × 8 plates, and that would be as many as you would care to make at one time. And, another thing, it is not possible to make two batches exactly alike. No amateur could do that. I do not think the manufacturers can do it, and the best way to secure an average speed in emulsions is to make a number of small batches and combine them. With four or five small batches combined just before coating, you will get an average speed that will not vary much. You may have a number of batches ripening all the time. When you want to make a few plates of an evening, take a teaspoonful from each glass and melt together.

Only photographic gelatine should be used. This has been cleansed of fatty matter. Nelson's No. 1 soft and Heinrich's, of Winterthur, hard gelatine are the best. Hard and soft gelatine may be cut up fine with scissors and kept in tin boxes ready for use. Gelatine should never be dissolved by adding it dry to warm ripened emulsion. It should first be allowed to swell for a few minutes, and then dissolved at a low temperature. The last addition of gelatine to the emulsion, after washing and remelting, should never exceed what is necessary to cause it to set. The amount of gelatine in the emulsion should be such that when a drop placed upon a strip of glass is held against a piece of ice to set, it will have a cheesy consistency when pressed with the finger. If it sets tough, like rubber, add water. I use equal parts of hard and soft gelatine all the year round for the final addition. Enough should be added to bring the total amount of gelatine in the finished emulsion up to thirty grains to the ounce.

The final addition of hard and soft gelatine having been made to the ripened emulsion, we proceed to filter it. Obtain an Argand chimney, which is simply a tube of glass about seven inches long by one and one-half in diameter. Upon one end of the chimney stretch a piece of chamois skin (not sheep skin) which has been dampened with water. Two or three turns of a strong rubber band will hold the chamois skin on. Warm the tube over a lamp, pour in the emulsion, then, placing the mouth close to the open end, blow vigorously, and the emulsion will be forced through.

The emulsion is now ready for coating. We next want some clean glass plates of the desired size. We may use old or spoilt negatives over and over again by thoroughly cleaning them.

After the old plates stand in a strong solution of washing soda, which is pretty near the boiling point, for ten or fifteen minutes, then, with a small scrubbing brush, the films may be easily removed. By placing small clips of wire or tin on the edges of the plates, they will be prevented from sticking together while in the soda solution. Have three basins of hot clean water handy, rinse the first plate in each immediately after removing the films, and stand in a rack to dry. When dry, polish the side to be coated with a piece of chamois skin moistened with alcohol, upon which a little whiting has been dusted.

A level surface upon which to allow the gelatine film to set is the next requisite. A slab of marble or slate will do for this. It may rest upon three screws set in the table or bench. A small spirit level should be used in adjusting it. The slab should be as cold as possible when used. A little chopped ice may be allowed to stand upon it for half an hour previous to coating, or it can be chilled in a refrigerator. It is important to have the film set as soon as possible after coating, as the sensitive bromide will have a tendency to settle down from the surface of the film, making long development necessary in order to get density. It also decreases its sensitiveness. The best method is to place the freshly coated plate upon a perfectly level shelf in a refrigerating box, but this is not absolutely necessary.

Plates larger than 5 × 7 seldom have a perfectly plane surface, being more or less "dished." The emulsion should be poured upon the concave side, and the four corners pressed down by weights before the emulsion has time to set. Of course a small spot on each corner has to be sacrificed when this is done, but plates having considerable curvature can thus be coated with a film of uniform thickness. As soon as the film sets, which it should do in three or four minutes, place the plate in the rack in the drying box. Use about one and a half drachms of emulsion to a 4 × 5 plate. A little practice will enable the amateur to pour about the right quantity upon the center of the plate without measuring. Guide the emulsion with a glass rod to the edges. Hold the plate on the extended fingers while coating, rocking it gently until the emulsion has spread uniformly. If the temperature of the room is below 65 degrees, the pile of clean glass should remain near a stove or in a warm place long enough to take off the shell before coating, but the plates should never be appreciably warm to the touch. The drying of the plates will be a serious trial to the patience if the proper arrangements are not made.

The best drying box for the amateur should be made of a tin cracker or cake box. A tinsmith will put on an extra rim which will make the cover light-tight. To each end of the box attach a three-inch tin pipe having at least two elbows to serve as light traps. Arrange a shelf or stand for the box near a chimney, and connect one pipe with the flue. A strong draught of air will pass through the box. The inside of the box and pipes should be blackened with a mixture of lamp black and alcohol, containing just enough shellac to keep it from rubbing off. By placing the ruby lamp, or any lamp, under the horizontal inlet pipe, a few inches from the box, the air is slightly warmed, and the drying is hastened. Do not let the temperature in the box exceed 86 degrees, or tough, glassy film will be the result. The plates should stand in a rack in the direction of the draught and at least three-quarters of an inch apart. A batch of plates will dry overnight in such a box. They should dry with a matte surface, and will show no drying marks if the emulsion has been thoroughly washed and the draught is strong. A piece of mosquito netting, smeared with a little glycerine or molasses, and made to cover the open end of the inlet pipe, will prevent dust from entering the box. This precaution is hardly necessary if there is no carpet upon the floor. As the amateur will probably make his plates in the evening, any room may be used that can be kept dark, but I advise him by all means to have a room, such as an unused chamber, attic, or storeroom, where he can leave his bench, chemicals, glassware, and drying box in dark and inviolate seclusion during the day. A good dark room is, of course, the best place, although if very damp the plates will dry slowly. Extreme care is necessary not to contaminate the emulsion with any of the chemicals used in developing. There is a great difference of opinion in regard to the use of iodide in emulsions, and this is due to the fact that slight changes in manipulation where it is used produce such widely different results. My own method of using it has given excellent results. In the first place, I find that three grains to every hundred of silver is as much as it is safe to use. I generally use about two grains.

Follow the formula previously given until ready to add the silver to the bromized gelatine. Before doing so add to the latter one drachm of a solution of iodide of potassium, containing about eight grains to the ounce. Do not shake the gelatine solution until you begin to add the silver. Proceed as directed in previous formula until the emulsion has digested at 100 degrees F. for one-half hour, or until the blue stage is reached; then add fifteen grains of swelled soft gelatine with which ten drops of ammonia has been mixed just before using. When this has dissolved, pour the emulsion into a dish to cool, but do not wash at once. Let it stand from ten to twenty-four hours. Then wash it, remelt, and ripen for one day or more. This is a very rapid emulsion, giving fine negatives of portraits, interiors, or landscapes.

In the above methods the bromide has a firm hold upon the gelatine before the iodide is added. If the bromide and iodide are added together, a modification of the double salt is produced, which requires long digestion and is very hard to develop and fix.

AN ABSOLUTE STANDARD SENSITOMETER.

SOME little time ago the writer determined to try and make an instrument which should test the sensitiveness of photographic films in such a way that it might be stated in terms of positive measure, and not, as heretofore, in reference to some arbitrary standard. "Ten times" and "twenty times" used as measures of sensitiveness *may* mean anything, they certainly mean nothing definite. "Rapid," "extra rapid," give some idea of the uses to which the plates may be put, but only vaguely, while the "rapid" of one maker probably differs widely from the plate of the same name by another maker. It seemed to the writer that photography had now attained a position when plate users might fairly say to plate makers, "Send me some plates of such and such a sensitiveness." Just as an engineer can say to a steel maker, "Send me some steel of such tensile strength;" or the electrician, "Send me some copper wire of such conductivity." It is true that there is a sensitometer in the market which is largely used, but it has an arbitrary scale, based on no natural scientific standard, and depends, so far as one instrument is comparable with another, upon the careful matching of a series of tints, which are liable to change, so that even if the same instrument be used, the readings of to-day are not strictly comparable with those of a month ago.

The writer experimented with various ways of forming tints, and found so much difficulty and uncertainty in getting anything that could be reproduced with certainty time after time, that he came to the conclusion that another principle of testing must be adopted. On thinking the matter over, it appeared to him that instead of varying the intensity of the light acting upon various portions of the plate, the better way would be to let light of the same intensity act upon various portions of the plate for different lengths of time. The result, so far as the action of the light is concerned, will evidently be the same, while the latter plan has the advantage of being possible to be repeated with exactness in any place and at any time, and also of more correctly representing the circumstances under which the plates would be used.

The principle of the machine is very simple, is based upon scientific truths, and may be stated in such terms that a test made in England is strictly comparable with one made in America, and one made to-day with one made one hundred years hence.

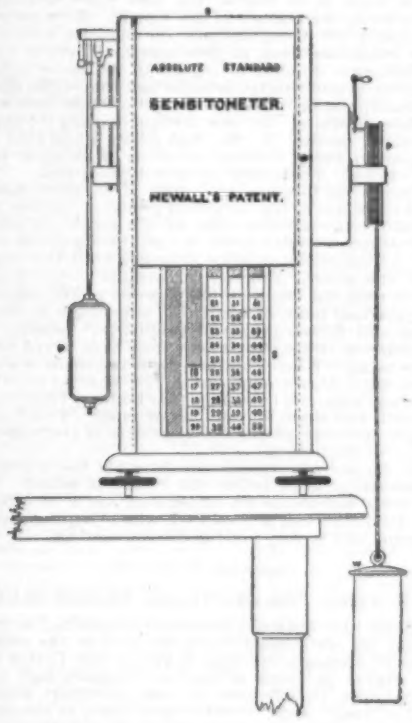
Briefly, the plan is this: A sensitive plate is placed at a fixed distance from a standard light, and small portions of the plate are exposed for gradually increasing periods of time to that standard light. Thus the terms necessary to express the sensitive value of the

plate are: (1) The light; (2) the distance between the plate and the light; and (3) the time that any portion of the plate was exposed to the light to produce when developed a visible image.

There is, of course, a somewhat uncertain point at either end of the operation, viz., the intensity of the standard light and the energy of the developing solution. But these are incidental to any system of testing, and will probably be improved, if not made practically perfect, in the near future, while the testing itself is based on natural laws, and is correct beyond possibility of doubt. In the meantime it would be well if the Photographic Society would decide whether a standard candle or Harcourt's pentane standard should be used as the standard light, and what solution should be used as the standard developer.

The apparatus devised to carry out this system of testing is as follows (it is made to test quarter plates, but can be made to take larger ones if wanted): B is a kind of box containing the mechanism; S is a very thin brass plate, in which are cut a series of figures from one to fifty. A series of shutters (one for each row of figures) works closely in front of the plate, and outside is one large shutter which closes up everything. P is a pendulum beating half seconds. D is a drum round which is wound a cord with a weight, W, at the end of it. This weight drives the mechanism. E is an escape wheel controlled by the pendulum, having a pinion fixed to it, which gears into a wheel on the arbor of the drum, D. Also on the arbor of D are five pinions, and on the backs of the five shutters are racks gearing with these last pinions. The pinions are so arranged that they come into action one after the other, each one starting when the one before it has lifted its shutter to the top. At each beat of the pendulum one of the shutters is lifted so as to expose one of the figures, beginning with fifty and ending with one.

The outside shutter, that is not shown in the sketch, is held up by a catch, which is released half a second



after the last figure on the plate has been exposed to the light.

The method of working is as follows: The instrument is supposed to be placed on a table in a dark room, so that the weight can fall freely the length of the cord. The weight is wound up and all the shutters let down. The weight drum is locked by a catch till all is ready to begin the test. The plate to be tested is placed behind the figure plate and pressed up by the spring back. The standard light is lit, and placed at the determined distance. The outer shutter is raised till held by the catch. Finally, the trigger is pulled, when the five shutters will rise one after another, in steps half a second apart, until all the fifty figures on the plate have been exposed, and then the outer shutter falls and prevents further action on the plate, which is then taken out and developed. Suppose the lowest figure visible to be twenty-five, its sensitiveness may be stated thus: "The plate will give a developable image when exposed to a standard candle (or pentane light) at a distance of say ten feet for twenty-five half seconds."

It will be seen that the terms of this statement are such that they can be used and verified by any person, in any part of the world, without the instrument used having been previously compared. In other words, it is a natural and absolute standard.

The instrument may also be used for another purpose. If a batch of plates be carefully standardized, then the instrument may be used to determine the chemical energy of various artificial lights, or of daylight on different days, on different times on the same day, or of moonlight. In the case of daylight it would be necessary to diminish the full intensity by limiting the size of aperture through which light got access to the plate. But again, all these values would be strictly comparable one with another in terms of time, distance, and size of aperture.

Many details of construction have necessarily been omitted in describing the instrument here, but if any reader wishes to know more about it, the writer will be pleased to answer any question that may be addressed to him.—*J. W. Newall, in Brit. Jour. of Photo.*

[Continued from SUPPLEMENT, No. 695, page 11108.]

ARC LAMPS AND THEIR MECHANISM.*

By Prof. SILVANUS P. THOMPSON, D.Sc., M.I.E.E.

THE most recent form of clutch wheel is that devised by Alexander Siemens, 6987[87], illustrated in Fig. 20. The lamp to which this device is applied is a holophote lamp for search lights, with a screw feed, the screw being turned by this clutch wheel. It is about one inch in external diameter, and has a projecting rim, R, within which lies an internal metal ring, B, slightly sprung outward, and cut obliquely, somewhat in the

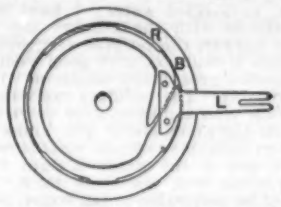


FIG. 20.—SIEMENS' CLUTCH WHEEL.

manner of the elastic metal packing rings used in engine pistons. The two ends of this internal split ring are joined, by two pins, to a small lever, L, which projects outward. When a reciprocating motion, of either large or small range, is imparted to the forked end of this lever, this internal clutch alternately bites against the rim and pushes it forward and then releases its grip and draws back. This device, which appears to be a really new mechanism, constitutes a true positive feed.

The same device reappears in a recent lamp by Fein [1888], which has a main circuit magnet striking the arc by drawing down the lower carbon, and a shunt magnet for feeding, which vibrates an armature by make-and-break arrangement. The upper carbon holder is smooth, of rectangular section, with a smooth feeding roller firmly pressed by a spring against it. This feeding roller is attached to a clutch wheel like Fig. 20. The lamp will work even upside down. Unlike the clutch wheel devices previously described, Fein's feed is non-retractile. The latest patterns of clutch wheel feeds are those of Mackie [7184[87]] and Mathis [1070[87]]. The advantages of the clutch wheel mechanisms over the clutches that act directly on the rods may be summed up by the remark that they render the operation of the lamp independent of dust and dirt.

Of lamps with *screw feed* motions, the most recent are the projector lamp of Siemens [6987[87]] mentioned above and a motor lamp by Maquaire, which is now being used at the Kensington Court lighting station. In the latter an ingenious and simple method of reversing the motion of the motor armature is adopted, so that both striking and feeding are accomplished by the same screw. This lamp is further described below.

The class of lamps in which cord and pulley mechanism is employed in conjunction with a long range open solenoid goes back to the early examples of Archerau

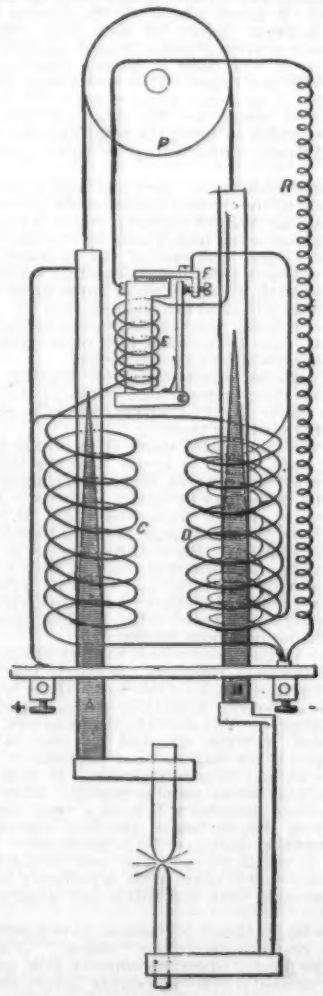


FIG. 21.—PILSEN LAMP.

and Jaspar. In the more complex Pilsen lamp [Krizik and Piette, 1397[88]] two solenoids are used, each with its own coned core, the plungers being connected by cord and pulley, and each bearing one carbon pencil. The arrangement of the circuits of the recent form of lamp is shown in Fig. 21, the core, A, being drawn into the main circuit solenoid, C, to strike the arc, while the differentially wound solenoid, P, varies its pull on the core, B, according as the carbons are required to approach, to stand still, or to recede, the former action occurring when the shunt current prevails. The special cut-out mechanism, E F, will be alluded to later.

A much simpler pattern of lamp has been several times described with various modifications in detail by Jaspar, 1878; Romanze [3901[88]], Andrews [3393[87]], and lastly by Menges, 1887, whose form is shown in Fig. 22.

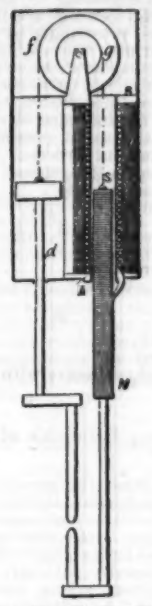


FIG. 22.—MENGES.

Here the long core, S N, is drawn up into the solenoid, wound differentially with a coarse wire, A, and a fine wire winding, B. A heavy piston of metal on the top of the upper carbon holder, *d*, serves both to counterpoise the weight of the core and to check, as it slides in the surrounding tubes, any too sudden movements. It is often claimed for solenoid lamps that the absence from them of all mechanism renders them superior to all other species of lamps. Yet they are not free from defects; they do not always burn steadily; some of them have the evil habit of "pumping;" and so simple a matter as the elongation of the cord which connects the plungers may put their regulating properties to confusion. In a recent modification by Doubrava the iron cores are fixed, and the solenoid coils slide up and down over them, giving motion to the carbons.

The *screw feed* lamps have been already touched on. Of these, the most successful in practice is the Cance lamp [8976[87]], which operates by the weight of the upper carbon. The screw is prevented from turning by a nut gripped until feeding is necessary. In Siemens' holophote lamp [6987[87]], and in Maquaire's new lamp [1889], the mechanical turning of the screw forces the carbons forward.

Among *magnetic clutch* lamps the Gulcher lamp, as improved by the Gulcher Company [Whiteman, 1915[87]], still holds its own. In this lamp the single counterpoised horseshoe electromagnet—with unequal limbs—sticks itself on by one pole to the iron carbon rod, and by the other pole attracts itself upward to strike the arc. For constant potential work, it is a very steady lamp. More recently, magnetic clutch wheels have been used by Dittmar [1886], and by Trott and Fenton [6910[88]].

Motor lamps have been devised from time to time, those of Tchikoleff, Schuckert, and Gray being perhaps the best until recently. The latest lamp of this type is that of Maquaire [1889]. In this lamp there is a Pacinotti ring upon a vertical pivot, on which a worm is cut. This gears into a large wheel, the pinion of which drives the upper carbon holder by a rack. The motor ring stands between the pole pieces of a powerful electro-magnet. By rotating it in one direction it strikes the arc; by rotating it in the reverse direction it feeds the arc. The direction of its rotation is reversed by reversing the direction of the current in it. This is accomplished in the following ingenious way: The coils of the electro-magnet are in the main circuit, and from a point midway along these coils a branch wire goes to one of the brushes of the armature. The other brush is connected to the tongue of a sort of relay, and stands between two contacts, one of which goes to one end, the other to the other end of the coils on the electro-magnet. By raising or lowering the tongue the armature is thus connected as a shunt, either to one-half of the magnet coils or to the other, the current in it being in opposite directions in the two cases. The tongue of the relay is controlled by a separate shunt coil, which attracts the tongue in order to feed the arc. The feed action of this lamp is extremely delicate.

It would be impossible, in the scope of this paper, to dwell on the *hydrostatic feeds*, on the various modes of producing *vibrating feeds* and *hammering feeds*, which have been from time to time suggested. The notion of having a *continuous* feed, the rate of which should be varied as required, is an enticing one, but it has never been made a practical success. Neither have the periodic methods of feeding, at which Mr. Brockie made so brave an attempt in 1881; the flavor of which indiscretion still clings about him in spite of the admirable lamps which he has produced in later years. Mr. Brockie's various forms of arc lamp would indeed well illustrate the whole range of the subject, and the same thing might be said about the numerous successive types of lamp produced by Mr. Crompton. Mr. Brockie has lately produced an excellent little projector lamp for use in the optical lantern, having the negative carbon pressed by spring against abutment screws of steel, while the upper carbon is fed forward by a nip-

* A lecture recently delivered before the Society of Arts, London. From the *Journal* of the Society.

ping lever controlled by a solenoid. *Abutment* lamps and *projector* lamps form separate classes by themselves, which cannot here be discussed.

Many of the points alluded to in the preceding descriptions are illustrated by the action of the De Puydt lamp, Fig. 23. This is a rack train lamp which strikes

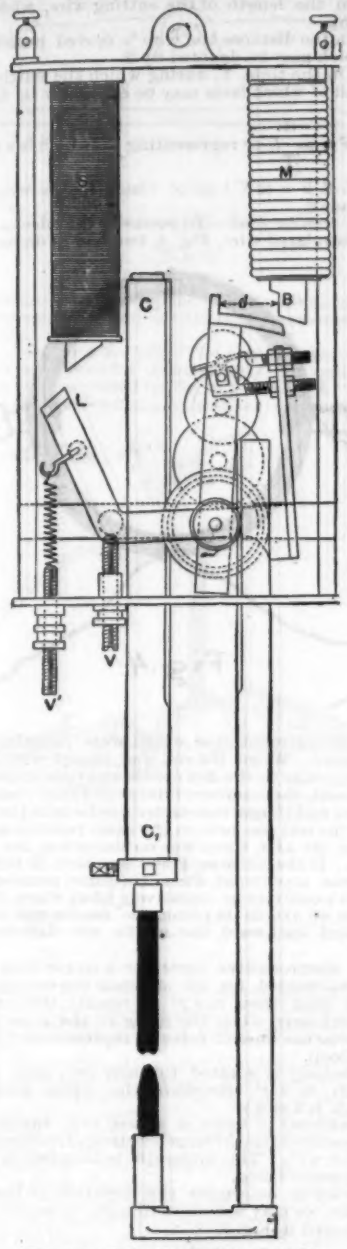


FIG. 23.—DE PUYDT'S LAMP.

its arc by lowering the lower carbon and raising the upper simultaneously, and feeds with a fly and detent controlled by a shunt magnet. The main circuit magnet, M, attracts obliquely its armature, rocking the train of wheels around the pivot of the first wheel. A lever, L, bent to an elbow which rests on the set screw, V, is also pivoted around the same arbor, and has a vertical arm which carries two attachments: a fork which engages on a pin on the train frame and a detent which arrests the fly. The other end of the lever, L_1, is held back by a spring, but can be attracted up by the shunt magnet, S, when the detent is to be released in order to feed.

PRODUCT REGULATION.

Before passing away from the feeding mechanisms, we may return to the question of regulation in general, and in particular to the point that the function of the mechanism of the lamp is to maintain constant one of the two factors of the electric energy, the dynamo being responsible for the other. That part of the electric mechanism which controls the feed is responsible for the steady working of the lamp; and it is this which has to be sensitive to the fall of the current (in constant potential lamps), or to the rise of the potential (in constant current lamps). It is this which must control the feeding mechanism of the lamp. It may be remarked that the striking electro-magnet represents the muscle of the lamp, but the feeding electro-magnet represents the brains. Now, if the brains of the lamp have to think about, and control, one factor in the product, why can they be not made to think about and control the product itself? Such a lamp, in which the controlling part is neither an ampere meter (main circuit) device, nor yet a volt meter (shunt) device, but a watt meter device, ought to make the lamp independent of any fluctuations in the supply. Product regulation is clearly in the abstract better than regulation either by the amperes alone or by the volts alone, or by the difference between them; and the mechanism is certainly such as can be constructed. Such a lamp ought to run on any circuit. Let it be remembered that on a constant potential circuit a shunt magnet can do no more than a spring might do. Neither can a main circuit magnet in a constant current circuit do more than a spring. A lamp in which both coils are properly combined ought to be capable of working on either kind of circuit. Indeed, there are some existing lamps which will very nearly do so.

RELAY LAMPS.

It is evident that the brains of the lamp need not necessarily be placed inside the lamp itself; the device of using a sort of *relay*, outside the lamp, to watch the lamp and control its feed, is one that has been frequently suggested [Crompton, Abdank, Gerard, etc.] There is one lamp [Jolin, 2,570[89]] in which a shunt relay, itself too weak to move the feed tilting plate, does the work by opening and closing one of the circuits of a double, differentially wound, main circuit electro-magnet.

RETARDING MECHANISMS.

It would be very easy to write a whole chapter on fans, air dash pots, glycerine dash pots, and the innumerable devices which have been tried. Two are worthy of mention, namely, Hopkinson's device [153[81]] of altering the rate of feed by raising or lowering mercury in a vessel in which a fan is continuously rotating, and Brockie's suggestion of a liquid brake wheel containing internal partitions, communicating by small apertures, so that a liquid could flow but slowly from one chamber to another.

REPLACEMENT MECHANISMS.

These may be classified as follows:

(*a.*) In rack train lamps, a ratchet wheel on first or second member of train.

(*b.*) In clutch lamps with loose clutches, none required.

(*c.*) In forward feeding clutch lamps, some device for loosening the clutch.

(*d.*) In screw feed lamps, some device for unclamping the screw gearing.

FOCUSING MECHANISMS.

The various ways of giving to the carbons the desired differential rates of approach are best classified by reference to Fig. 24, which gives four methods of pro-

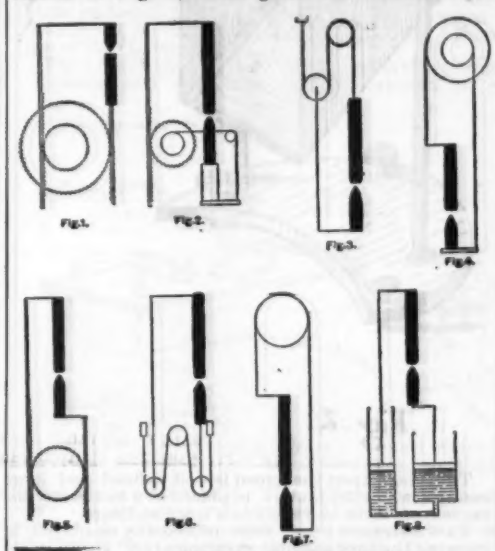


FIG. 24.—MODES OF FOCUSING.

ducing a motion of the carbons in the ratio of two to one, and four methods of "commercial focusing," in which the carbons are moved toward one another at equal rates.

CHANGE-OVER MECHANISMS.

These may be classified into those which are purely mechanical [Brush, Thomson-Rice, Burgin, Crompton-Crabb, etc.], and those which are electrical, the current switched off from one pair of carbons and switched on to another [Weston, 1,163[82]; Waterhouse, 5,185[81]; Trott and Fenton, 6,910[87]; Noble, 16,376[87]; etc.]

CUT-OUT MECHANISMS.

"Cut-out" mechanisms may also be purely mechanical; but they are more usually electrical, and, in modern forms, enable the lamp to "cut in" again, on the re-establishment of its proper circuit. Cut-outs which enable a shunt magnet to strike and feed the arc are described by Lever [8,443[84] and 11,501[84]], and are used in the Thomson-Rice lamp. The latest devices are those of Lauckert [5,707[87]], and Phillips and Harrison [7,235[87]]. In many lamps the cut-out circuit is merely a shunt electro-magnet stiffly set, which on the failure of the lamp, receives the whole current, and pulls over a cut-out switch. Gerard [4,792[87]] describes several such devices, as well as a cut-out depending on expansion by heat. In the Pilsen lamp (Fig. 21) the cut-out is in an auxiliary main circuit. Cut-outs depending on the fusion of a wire or of a washer have been proposed

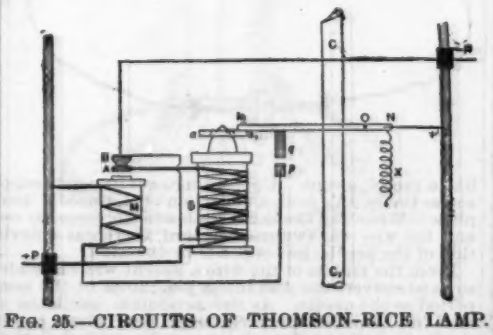


FIG. 25.—CIRCUITS OF THOMSON-RICE LAMP.

by Lontin [2,004[87]], Basilevsky [5,020[84]], and McDill [1887].

The action of change-over and cut-out mechanisms is well illustrated by reference to the Thomson-Rice lamp, of which Fig. 25 illustrates the electrical mechanism, and Fig. 26 the mechanical mechanism of the lamp. The terminals are marked + P and — P. The carbon rods, C C and C_1 C_1, are held up by their respective clutches, which are identical with those of the Thomson-Houston lamp (Fig. 14) already described. The clutches, when the lamp is not at work, are them-

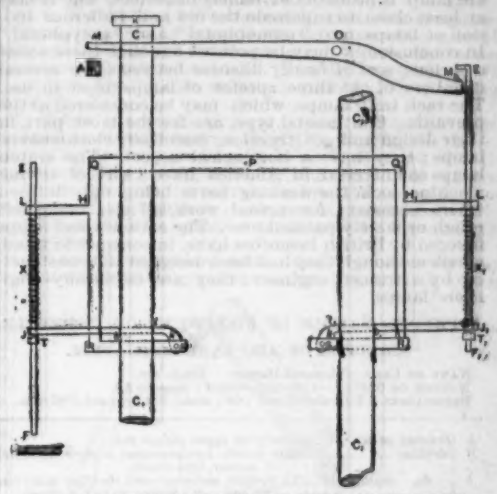


FIG. 26.—MECHANISM OF THOMSON-RICE LAMP.

selves held up by the spiral springs, X X, which push down the levers, J and J_1, and push up the arms, H L and H_1 L_1, so gripping the rods. The principal electro-magnet, S, with its conical poles, has a pierced armature, *a a*, mounted on the end of a strong lever, M N (Fig. 25), which is pivoted at O, the short tail, O N, being pulled down by a powerful spring, X. This lever thus held up above the poles of the electro-magnet is connected to point, P (Fig. 26), of the clutch frame, and holds it up. The adjusting nuts of this frame are so set that when P descends the lower end, F, of the pin, E F, strikes on the base plate and opens the clutch for the left carbon rod, so striking the arc of this pair of rods. When the pencils of this side are consumed, the rod, C C, has descended so far that its projecting head, K, strikes the long lever, M M, the other end of which raises the pin, E F, so bringing the other clutch into action. The electric arrangements, Fig. 25, are as follows: The magnet, S, is wound with a few turns of thick wire and with many turns of fine wire. The cut-out (or cut-in) magnet, M, is wound with coarse wire only. The upper carbon rod is connected electrically to the frame of the lamp, while the lower carbon, insulated from the frame, is joined by a wire (not shown in the figure) to the negative pole. The current entering at + P passes first round the coarse wire on S, most of it going through A and B (which are in contact), and on to — P; but a little of it, after traversing the coarse wire coil, turns back along the fine wire coil and goes by this by-pass route to B and — P. As a result, the armature is strongly attracted, and the lever, M N, descends, causing the carbon rod to drop. This now opens another route for the current. Starting at + P, some of it will flow through M to the frame, thence through the upper and lower carbons and so to — P. This current will attract the armature A, and break the contact between A and B, breaking the coarse wire circuit through S, leaving the fine wire path still available. Consequently S loses most of its power,

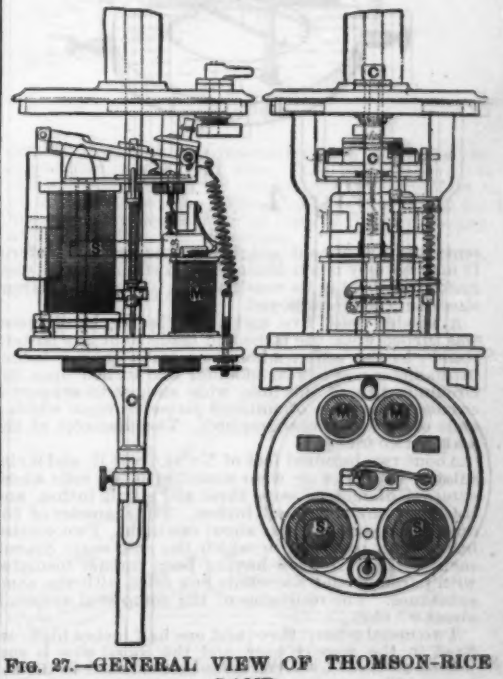


FIG. 27.—GENERAL VIEW OF THOMSON-RICE LAMP.

M. The cut-out (or cut-in) electro-magnet which opens the contacts of the coarse wire winding on the electro-magnet, S. S. The principal electro-magnet, wound with fine and coarse wire. The armature of this electro-magnet is attached to a lever pivoted at O, and held at its other end by a strong spiral spring. This armature, in sinking, opens the clutch, and, in rising, closes it on the carbon rod, which it raises.

and the spring × pulls up the lever, M N, so striking the arc. The shunt circuit (fine wire) through S is still at work, and continues to operate the feeding, increasing as the resistance of the arc lengthens.

The present attempt to furnish a classification of the various mechanisms, electrical and mechanical, of the arc lamp is probably extremely imperfect, but it may at least claim to supersede the old and ludicrous division of lamps into "monophotal" and "polyphotal." In conclusion, it may be pointed out that there exists a curious sort of family likeness between the several members of the three species of lamps most in use. The rack train lamps, which may be considered as the prevailing Continental type, are for the most part, in their design and construction, essentially clockmakers' lamps; they have a horological aspect. The clutch lamps so universal in America have a sort of sewing machine look, the working parts being only finished where necessary for actual work, all else being left rough or merely painted over. The clutch wheel lamps favored by British inventors have, in contrast to these, a look as though they had been designed and constructed by a trained engineer; they are essentially engineers' lamps.

APPENDIX—SAMPLE OF FILLING UP OF SCHEDULE.

SCHEDULE OF ARC LAMP MECHANISM.

NAME OF LAMP.—Thomson-Houston. Single arc.
NATURE OF SUPPLY.—Constant current; *amperes* 9·8.
RESISTANCES.—*Main circuit coil*, 0·09; *shunt*, 279; *cut-out*, 0·85 ohm.

A	*Driving power*	Gravity on upper carbon rod.
B	*Striking*	Main circuit electromagnet attracts lower end of seesaw, lifts clutch.
b	*do. adjustment*	(1.) Special resistance coil shunting main circuit coil. (2.) Spring on tail of clutch.
C	*Feeding*	Shunt electromagnet attracts upper end of seesaw, lowering clutch.
c	*do. adjustment*	Same as *b*.
D	*Moderating*	Air dash pot on horizontal tail of seesaw.
E	*Replacement*	None required.
F	*Focusing*	None.
f	*do. adjustment*	None.
G	*Change-over*	None.
H	*Cut-out*	Insulated contact on lower end of seesaw, pulled over by shunt coil, and puts cut-out resistance as shunt from + to — terminal.

A NEW GALVANOMETER FOR PROJECTION.*

By Prof. J. W. MOORE.

THE galvanometer about to be described has been used for many years and has given great satisfaction.

It is small, compact, cheap, and sensitive, and can be used apart from the lantern. Experiments which ordinarily require large and expensive apparatus may be well shown in connection with the ordinary lecture table instruments of the shops.

It is the well known two-coiled galvanometer adapted to the American form of vertical lantern. It was found that this lantern would project an object upon the screen if an aperture of only one inch and a half was allowed at a height above the stand of three and one half inches. Advantage was taken of this fact to adapt the galvanometer for projection. Fig. 1 repre-

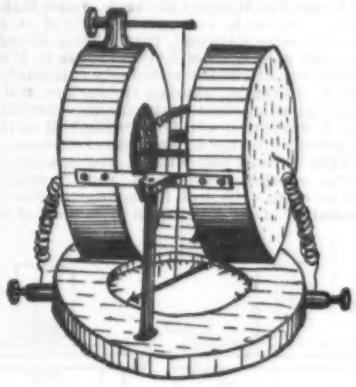


Fig. 1.

sents the instrument apart from the vertical lantern. It may be very much diminished in size if brass is used instead of wood in its construction. Wire of different sizes can also be employed.

A wooden base five and a half inches in diameter was turned upon the lathe and made to fit the lantern stand; in the center a hole two and a quarter inches in diameter was cut; a shoulder was turned upon the circumference of this hole, wide enough to support a circular glass plate of suitable diameter, upon which a scale of 360° was photographed. The diameter of the scale is two inches.

About two hundred feet of No. 14 (0·064 B. and S.) insulated copper wire were wound into two coils whose external diameters were three and a half inches, and thickness one and a half inches. The diameter of the opening in each coil was about one inch. Two wooden boxes were provided in which the coils were placed, each spiral of the coil having been further insulated with paraffine and the whole box filled with the same substance. The resistance of the completed system is about 0·5 ohm.

Two metal pillars, three and one half inches high, are fixed in the wooden base, and the coiled wire is supported on these by metal cross bars screwed to the coil boxes. The boxes are separated a distance of one inch and a half.

On the top of one of the boxes a small standard is placed which holds a horizontal adjusting rod. This rod has a motion of translation, also of rotation. The magnetic part of the apparatus consists of a piece of steel knitting needle, one inch and a half in length, strongly magnetized. This needle is attached to a fiber of unspun silk, the upper end of which is fastened to the extremity of the adjusting bar. Since the needle is out of focus with the photographed scale, a light, stiff wire, at the lower end of which is attached an arrow made of thin writing paper, is fastened to the center of the needle and so adjusted that the paper arrow and the scale are projected upon the screen at the same time. The combined length of the needle suspension is about six inches. The length of the needle is in a horizontal plane passing through the centers of the two coils. The ends of the coil wires are permanently attached to two binding posts, screwed into the wooden base, for convenience in the attachment of wires. Fig. 2 represents the galvanometer placed upon the lantern stand.

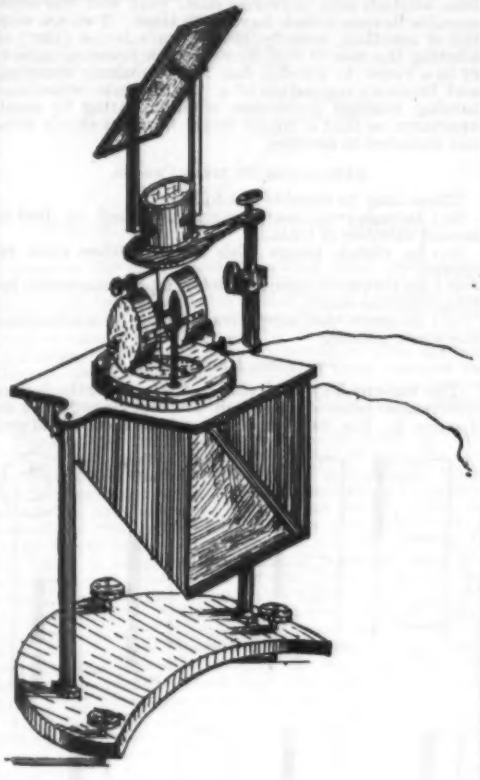


Fig. 2.

The image upon the screen is well defined and large enough, when the lantern is placed at a moderate distance from it, to be visible to a large audience.

This instrument has been principally employed in showing the fundamental experiments of electro-magnetic induction.

In 1861 Faraday's magnificent discoveries were made, which may be embraced in the following statement:

If a magnetic (including electro-magnetic *) field be cut across by a moving conductor or the field of a conductor by a magnet (including an electro-magnet) an electromotive force will be set up in the conductor when the number of positive lines of force embraced by the conductor is increased or diminished by the cutting.

A.—UNIFORM FIELD.

1. *Straight Conductor.*—The field of force surrounding a magnet may be uniform or variable. The lines of magnetic force surrounding the earth, in a limited space, are straight, parallel, and equidistant. The field, therefore, is uniform. Their direction is determined by that of the dip needle, which places itself tangent to them. The earth, therefore, may be used to prove the truth of Faraday's statement.

An insulated copper wire was stretched from one side of the lecture room to the other, a distance of thirty-five feet, and connections made to the galvanometer as represented in Fig. 3. The wire hung loosely

Fig. 3.

like a jumping rope. Upon swinging it in one direction across the earth's field, a deflection of the needle took place. When the needle was allowed to come to rest and the wire was swung backward, there was a deviation of the needle, but opposite in direction.

From the middle of the wire a weight was suspended so as to convert the wire into a pendulum of the same period as the needle. As the pendulum oscillated to and fro, impulses properly timed were given to the needle, and the deflection increased greatly in amplitude. The original number of lines of force embraced in the circuit was diminished by swinging the wire toward the galvanometer, and increased when swung from; therefore, momentary currents were induced.

It is evident that if the wire were moved so as to cut the earth's lines at right angles, the number of lines cut, and therefore the electromotive force set up, would depend:

1st. Upon the number of lines in the field, that is, the strength of the field, which may be called m.

2d. On the length of the cutting wire, which l may represent.

3d. On the distance the wire is moved parallel to itself, which may be denoted by S.

4th. On the time, T, during which the cutting takes place, all of which facts may be expressed in the equation

$$E\,M\,F = m\frac{S}{T}l, \text{ or representing } \frac{S}{T} \text{ (which is a velocity)}$$

by V, $E\,M\,F = m\,V\,l$, all of which truths were proved by Faraday.

2. *A Circular Coil.*—To intensify the effect a circular coil of insulated wire, Fig. 4, two feet in diameter, was

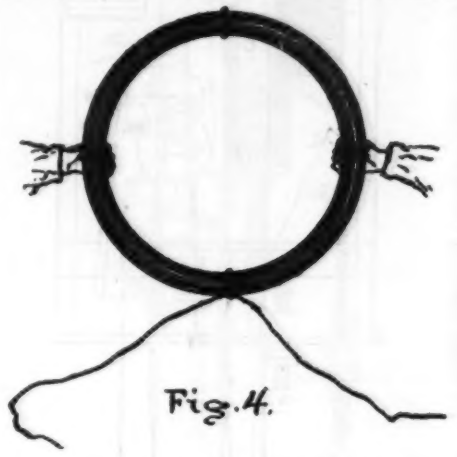


Fig. 4.

used, the extremities of which were joined to the galvanometer. When the coil was placed with its plane perpendicular to the dip needle and then moved parallel to itself, the number of lines embraced remained unchanged and the galvanometer needle failed to respond. When the coil was held in the same position and rotated upon its axis, there was no deflection, for the same reason. If the coil was given a motion of translation, there was no current, since the same number of lines was cut positively as negatively; but when it was rotated on an axis in its plane, the needle was deflected; if rotated backward, the needle was deflected oppositely.

The electromotive force for a single loop can be easily calculated, for let m equal the strength of the earth's field, then $m\,\pi\,r^2$ represents the number of lines embraced when the plane of the loop is perpendicular to the lines of force—r representing the radius of the loop.

If the loop is rotated through 180°, each half cuts through $m\,\pi\,r^2$, therefore the whole number cut through is $2\,m\,\pi\,r^2$.

If a number of loops, n, is used as in the experiment just described, the average electromotive force is equal to $n\,(2\,m\,\pi\,r^2)$. This apparatus is the same in principle as Delezenne's ring.

In order to determine the direction of the induced currents, we may use the ordinary *memoria technica* represented in Fig. 5.

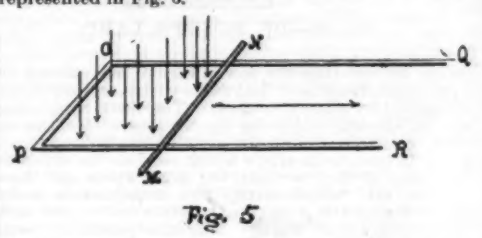


Fig. 5

Let O Q and P R represent two conducting rails in electric communication by means of P O. Imagine M N to be a moving axle. The arrows represent the lines of force of the earth. Now, if the observer looks downward in the direction of the lines of force and M N is moved to the right, the number of lines embraced by the circuit will be *increased*—an inverse current will be induced—that is, in the direction M N O P, or contrary to the direction of the hands of a watch. If M N is moved toward the left, the number of included lines will be diminished, a direct current will be induced in the direction N M O P, or clockwise.

Ampere's method may be modified to apply to all cases, as follows: Look in the positive direction of the lines of force, imagine yourself identified with the conductor; if you and the conductor are moved to the right, the number of lines of force embraced by the circuit will be increased, hence the current will flow from the feet to the head; if the motion is toward the left, the induced current will flow from head to feet.

B.—VARIABLE FIELD.

1. *Straight Conductor.*—When two insulated wires, Fig. 6, about fifteen feet in length, were stretched near each other, but not touching, and a current passed through one from the battery * used in experiments upon "lines of force," a distinct deflection of the needle of the galvanometer, connected with the other, became evident; when the current was broken, an opposite deflection occurred.

If the makes and breaks were timed to the oscillations of the needle, it was caused to swing through many degrees. When the circuit is closed, a field of circular lines grows around A B.†

On account of the nearness of C D, the galvanome-

* An expansion of a paper presented to the A. A. A. S., at the Cleveland meeting.

* SCIENTIFIC AMERICAN SUPPLEMENT, Nos. 642, 643, 644.

* SCIENTIFIC AMERICAN SUPPLEMENT, No. 642, p. 10256.

† SCIENTIFIC AMERICAN SUPPLEMENT, No. 642, p. 10256, Figs. 3 and 3a.

ter circuit embraces some of these, and a current is induced. When the battery circuit is broken, the field surrounding A B dies out, and hence the lines embraced by C D have no existence, that is, a less number is embraced. This illustrates the difficulties met with

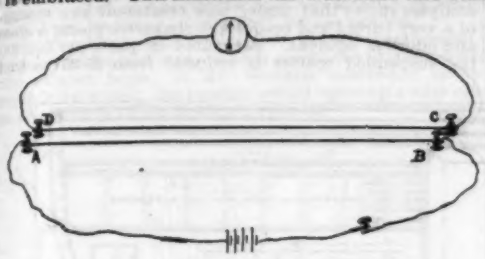


Fig. 6.

from induction on telephone lines in connection with the Morse system, and the alternating currents of light machines.

2. *A Loop and a Coil.*—The usual lecture table apparatus employed for illustrating electro-magnetic induction is represented in Fig. 7, and called a "dissected coil." The electro-magnet fastened to the base is the

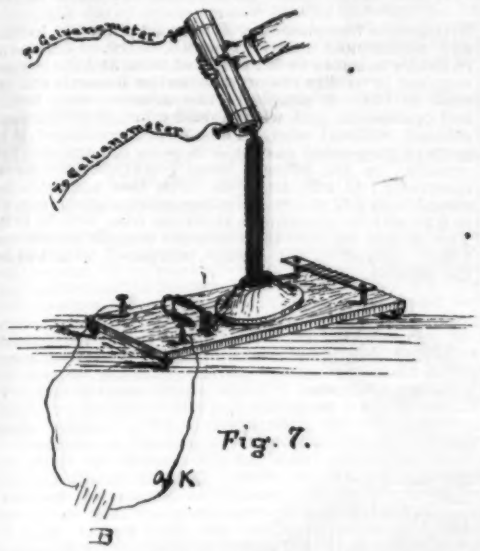


Fig. 7.

primary coil; the coil held in the hand is called the secondary. The former consists of a few turns of thick insulated wire, the latter of many turns of insulated thin wire.

Let the secondary coil be entirely removed, and then pass the current of the battery, B, through the electro-magnet. A reference to Figs. 23 and 29, SCIENTIFIC AMERICAN SUPPLEMENT, No. 644, shows that lines of force spring up from the upper extremity like the branches of a tree, which turn down and meet the lines striking out from below like its roots. Pass the current so that the upper end is a N pole—the plus direction of the lines will then be from the N, upward, outward, and downward, toward the S pole.

If now the simple loop, connected with flexible wires (Fig. 8) to the galvometer, be brought down over the top of the electro-magnet, a sudden slight deflection of

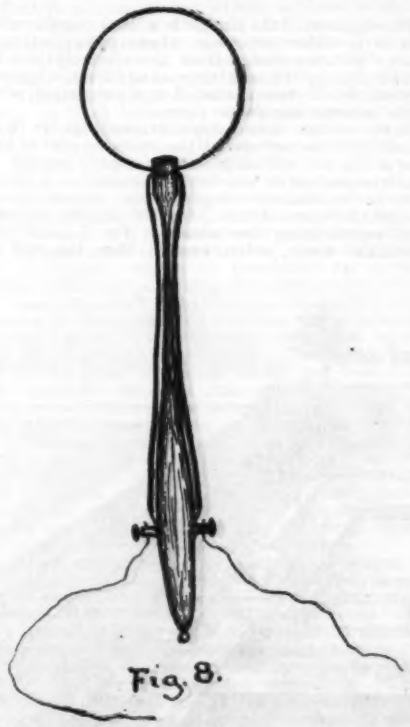


Fig. 8.

the needle will be observed; if the loop be stopped at the middle, the needle will come to rest. If now the loop be suddenly removed, the needle will be deflected in the opposite direction, and will again come to rest.

If the approach and recession of the loop are timed to the swing of the needle, a very large displacement may be observed.

It is obvious that when the loop approaches the electro-magnet a greater number of lines of force is embraced in its circuit, and, upon its withdrawal, a less number; hence the experiment proves—

1st. That a current is induced upon both the approach to and withdrawal from a constant current of a conductor.

2d. That these currents are instantaneous.

3d. That they are opposite in direction to each other.

The effects may be greatly intensified by disconnecting the loop (Fig. 8) and connecting the secondary coil (Fig. 7) to the galvanometer. On approaching the electro-magnet—slipping it over the electro-magnet—a violent push is given to the needle in one direction, and, upon suddenly withdrawing it, a violent deflection in the opposite direction. The needle may be caused to violently rotate.

It is desirable to know the direction of the induced current in the loop.

In order to determine this it is necessary to imagine one's self fixed in a certain position, relatively to the electro-magnet and loop. As good a convention as any is to look in the positive direction of the lines of force (*i. e.*, the direction a free N magnet pole would be urged; upward in Fig. 3). Now, holding the loop in a horizontal plane and looking upward from the electro-magnet, if the loop approaches, the number of positive lines embraced will be *increased*. A current from right over to left, anti-clockwise, will be induced, that is, an *inverse* current.

If the loop is withdrawn, the number of plus lines will be *diminished* and a *direct* or clockwise current will be induced.

This is practically the same as the method already given above.

3. *Primary and Secondary Coils Fixed Relatively to Each Other.*—When both the electro-magnet and conductor are at rest relatively to each other, so that no motion is possible to either, the number of lines of force embraced by the conductor may be increased or diminished by increasing or diminishing the magnetism of the electro-magnet.

Fig. 9 is sufficiently clear. It represents the ordinary "dissected coil" coupled up with two batteries, B_1, B_2,

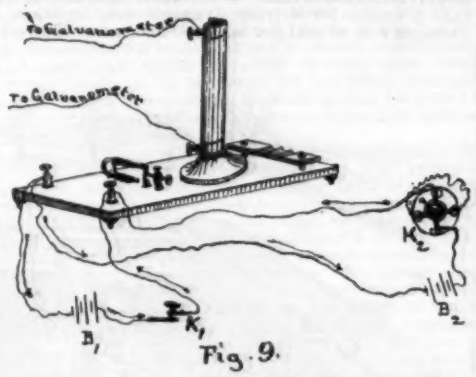


Fig. 9.

and a key in each circuit. The secondary coil is placed over the primary, and connected with the galvanometer. It is obvious that when no current passes there is no field. Allow K_2 to be opened. Immediately the circuit is closed by the key, K_1—the lines of force grow up around the electro-magnet, and the secondary coil embraces its maximum number; hence *closing* the circuit here is equivalent to *approaching* the electro-magnet with a movable coil.

If the circuit is broken by K, the branches and roots of the magnetic tree wither and disappear, and the number of lines embraced now is *nil;* the needle is deflected in the opposite direction. *Breaking* the circuit is therefore equivalent to *withdrawing* the conductor from a fixed field; *hence, as the books put it, a current is induced when the primary current is made or broken.*

The direction of the current is obviously determined as before. This is the principle of the inductorium.

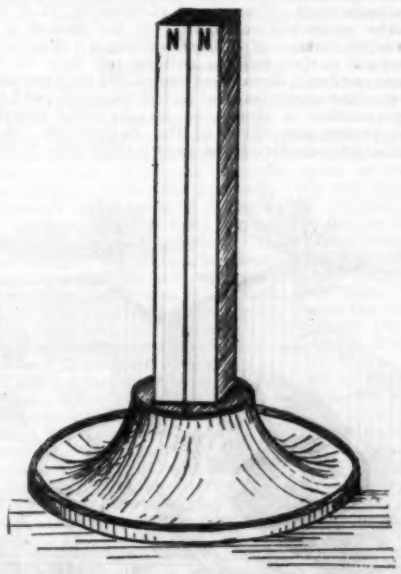


Fig. 10.

If K is kept closed and the commutator, K_3, manipulated, it is obvious that the current in the primary coil will be increased and diminished in strength according as the current of the battery, B_3, is sent in the same or opposite directions to the current of B_1. If the primary current is strengthened, additional lines of force will grow up and be embraced by the secondary coil; if weakened, a less number will be included. In each case a momentary induced current will be made evident by the galvanometer.

The ordinary text-book statement is that currents may be induced by increasing or diminishing the strength of the primary current.

4. *The Field of a Permanent Magnet.*—Since a permanent magnet, as regards its external effects, is the equivalent of an electro-magnet,* two ordinary steel magnets, each nine inches long by one half an inch square, Fig. 10, were substituted for the electro-magnet in Fig. 7.

When the magnets were placed with their north poles up so as to have a field like that of the electro-magnet, exactly similar effects were produced with the loop, Fig. 8. On approaching, the needle was deflected in one direction, on withdrawal in the opposite direction; the currents were momentary, and their directions in the wire could be determined by the usual convention.

When the secondary wire of the "dissected coil" was connected with the galvanometer, Fig. 11, and used in

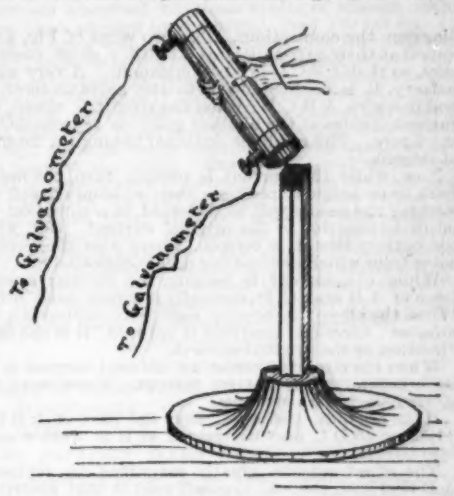


Fig. 11.

the same manner as the loop, more striking results were exhibited.

The method of expressing these facts in the text-books is that a current is induced in a conductor when it is brought up to or withdrawn from a magnet.

This experiment illustrates the principle of construction of the magneto-electric machine.

Fig. 12 illustrates an interesting form of experiment. Remove the secondary wire from the dissected coil,

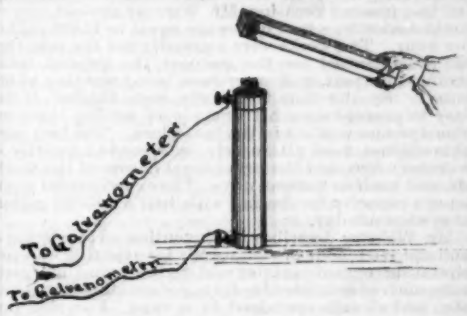


Fig. 12.

connect it with the galvanometer, and introduce into it a bundle of soft iron wire. It is obvious that if in any way the wire can be magnetized, a field will grow up around it, and the surrounding conductor will embrace an increased number of lines of force—if demagnetized, the lines will disappear.

To effect this result, bring up near to the bundle the

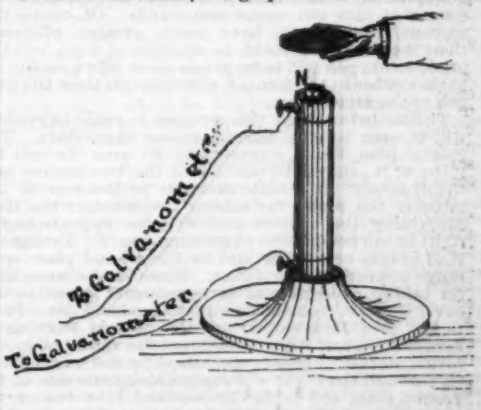


Fig. 13.

two magnets of Fig. 10. Instantly the needle is deflected; withdraw the magnet, the needle flies in the opposite direction; hence, as the books put it, a current may be induced by the production or destruction of magnetism.

Fig. 13 represents another form of the experiment. It is evident that if the production and destruction

* SCIENTIFIC AMERICAN SUPPLEMENT, No. 644.

of magnetism can induce a current, an increase or diminution of magnetism already existing will produce the same effect.

In Fig. 13 the bundle of soft wire is removed and the two magnets are put in their place.

If a piece of soft iron, as in the figure, or a bundle of soft iron wire be brought near and withdrawn from N, the magnetism of N will be altered and the needle will exhibit the effect by moving to the right or left.

This is the fundamental experiment in the telephone receiver of Bell.

SELF-INDUCTION.

The phenomena of self-induction can easily be exhibited by this galvanometer. Fig. 14 represents, in

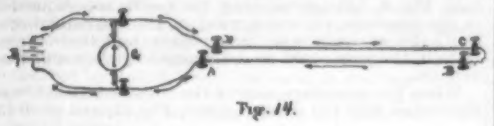


Fig. 14.

diagram, the connections. The two wires of Fig. 6 are joined at their extremities, B C, with a short piece of wire, so that A B C D is now continuous. A very weak battery, B, is coupled up with the galvanometer, G, and the wire, A B C D; when the circuit is closed, the current divides at the binding posts as represented in the figure. The needle is deflected through a number of degrees.

Now, while the current is passing, turn the needle back to its original position, then suddenly *break* the circuit; the needle will be deflected in a direction opposite to that due to the original current. But, when the battery circuit is broken, where does the current come from which deflects the needle oppositely?

While the current is passing, the circular lines of force of A B and C D mutually embrace each other. When the circuit is broken, each part embraces a less number; therefore a current is induced. It is the same direction as the original current.

When the circuit is made, an induced current is set up opposite to the battery current. These were formerly called extra currents.

It is obvious, therefore, that the part of A B C D adjacent to D C may be treated as if it were a separate circuit.

The effect may be greatly magnified by replacing A B C D with the coil so much used in that system of electric gas lighting in which the extra current on breaking the circuit is employed.

Lafayette College, Easton, Pa.

THE ELECTRICAL TREATMENT OF SEWAGE.

MR. WILLIAM WEBSTER recently gave a practical demonstration of his electrical method for the purification of sewage, at the main drainage outfall at Crossness. Permission was obtained from the Metropolitan Board to erect temporary experimental works on a piece of vacant ground at Crossness, the expense being defrayed by the inventor, who had his sewage for nothing, except that he bore the cost of pumping. On the present occasion Mr. Webster showed how he could deal with a flow of sewage equal to 12,000 gallons per hour. The works were avowedly not the best that could be devised for the purpose, the ground being limited in extent, and experience being wanting at the outset. By the time the works were finished, it was easy to propose some better mode of setting them up. The dynamo was not in the best place. The two portable engines, used alternately, were made a quarter of a century ago, and the provisional nature of the works showed itself in various ways. The experimental works have a capacity for dealing with half a million gallons of sewage per day.

Mr. Webster describes his invention as consisting of an improved mode of electrolysis for oxidizing and precipitating organic matter and decomposing inorganic salts, such as chloride of sodium, potassium, magnesium, etc., and all salts contained in sewage. For this purpose, in treating sewage and similar impure liquids, he subjects the liquid, while flowing through channels or reservoirs, to the electrolytic action of positive and negative electrodes made of iron and having very extended surfaces. Ammonia and other alkalies are evolved at the negative electrode, and assist the precipitation of the iron salts formed at the positive pole, where at the same time nascent oxygen and chlorine are evolved, partly acting on the iron and producing an acid reaction whereby the organic impurities in suspension are precipitated, while those in solution are oxidized and converted into innocuous compounds. Of course the nascent chlorine will have much greater efficiency than the chlorine present in chloride of lime, and the nascent oxygen will have much more effect on the organic matter in the form of albumenoids than the oxygen of the atmosphere.

The mode in which this process is made to operate will be seen by the accompanying engravings. The ground plan, Fig. 1, representing an area of about 250 ft. by 60 ft., shows the position of the two engines and the dynamo. At a little distance to the rear of the latter is the pump for raising the sewage; the shed containing the engines and dynamo appears on the right in the perspective engraving, Fig. 2. Facing the shed in this engraving and in the ground plan are a range of precipitating tanks. These tanks were used for taking experimental measurements, so as to discover the best mode of arranging the electrodes. Practically they form no part of the present working arrangements, and they would not be repeated in any future works. The sewage raised by the pump passes on through the shoot which runs along one side of the ground plan, and is finally received into one or the other of the settling tanks. A portion of the shoot appears on the right hand side in Fig. 3 opposite the implement shed. Between the shoot and this shed appear the tanks, as on the plan. In Fig. 2 another portion of the shoot is seen opposite the engine shed, with the precipitating tanks intervening. The shoot is an electrolytic channel in which are the iron electrodes arranged as shown in the small sectional drawing, Fig. 4. The shoot is made of wood, but in any permanent work would be constructed of some stronger material. Owing to the restricted space, the shoot at Crossness is fitted with wrought iron plates, but in practice it is intended to make use of cast iron, run direct from blast furnaces. As the sewage travels along the shoot, every particle of the liquid comes into contact with the plates, and is thereby subjected to the electrolytic action. A separation of the solid particles from the actual liquid speedily shows itself, and increases in its intensity as the sewage approaches the settling tank, into which it ultimately descends, and where the separation reaches its final stage.

Each of the portable engines is of 20 horse power.

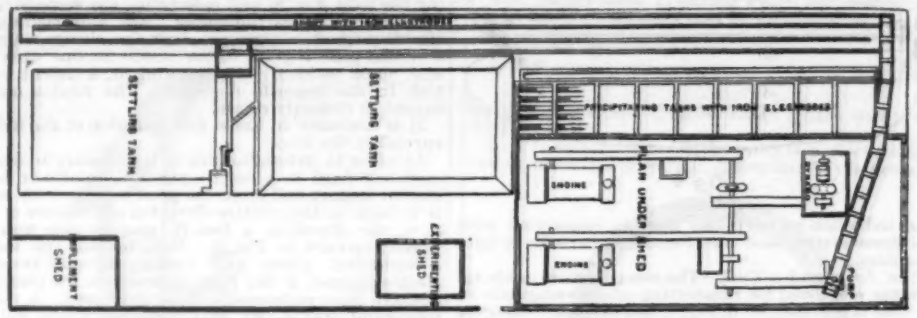


Fig. 1.—GROUND PLAN OF EXPERIMENTAL TANKS.

Only one engine is used at a time, both for pumping the sewage and for driving the dynamo. The dynamo is an Edison-Hopkinson, capable of developing an energy of 43 horse power. From the dynamo the leads run through resistance frames, by means of which the amount of current passing on to the electrodes can be regulated without varying the speed of the engine. From the statements made by Mr. Webster it appears that with 27 horse power it is possible to treat one million gallons of sewage in twenty-four hours, presuming the sewage to be of the same strength as average London sewage. In estimating the cost of the engine power, reference is made to the fact that the newest type of engine for driving dynamos may be taken to consume 2 lb. of coal per horse power hour. The price

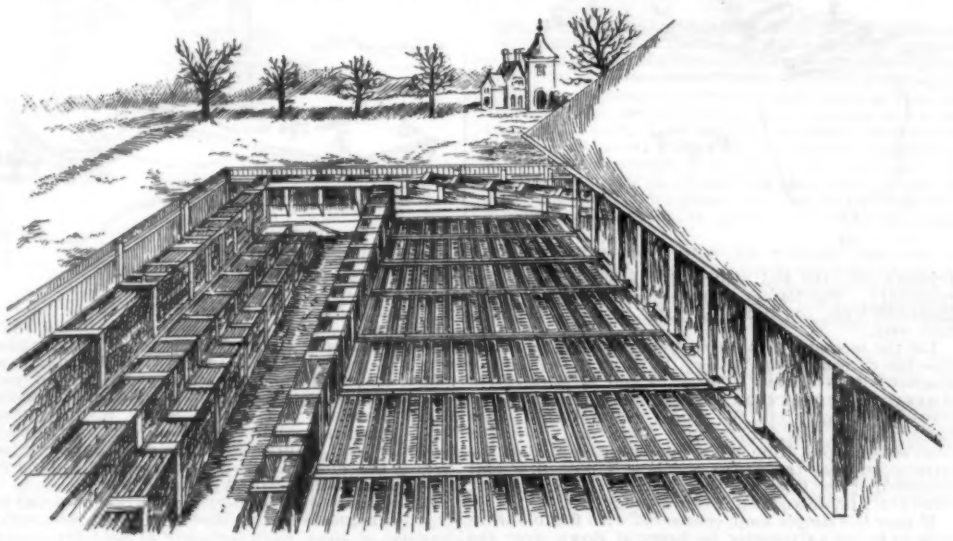


Fig. 2.—UPPER PORTION OF SEWAGE CHANNELS.

of coal regulates the result. As for the iron consumed in the process, the experiments tend to prove that the consumption in continuous working should not exceed two grains per gallon of sewage. Plates 1 in. thick, if used in sufficient numbers, would last for many years when once fixed. By way of illustration it is stated that the consumption of iron in the case of a town with a population of 333,000, having a volume of sewage equal to ten million gallons per day, or thirty gallons per head, should not exceed 464 tons per annum. The amount of mechanical power required per head of the population is shown to be $\frac{1}{1000}$ horse power, or 8 horse power per 10,000 of the population. A very small electro-motive force, only a little over 2 volts, is used, and about 11 square feet of iron electrode surface per ampere. There seems to be no polarization of the electrodes in the channels.

Running from the shoot, and allowing one hour for settlement in the open tanks, the average of twenty analyses shows that under this treatment raw sewage of a very turbid and opalescent character yields a clear and odorless effluent. Estimated in parts per 100,000, the suspended matter is reduced from 33·35 to 1·56. Nitrogen as free ammonia is reduced from 4·34 to 3·22, and albumenoid matter from 0·5 to 0·2. Chlorine as chlorides becomes 18·62 instead of 21·64, and the oxygen required to oxidize the organic matter becomes 0·52 instead of 1·24. A sample of raw sewage, very turbid and opalescent, and with a bad odor, yielded a clear effluent without odor. The sewage contained 14·53 parts of suspended matter, 5·95 being mineral and 8·57 organic. In the effluent these quantities became respectively 1·48, 1·05, and 0·43. The free ammonia declined from 3·57 to 2·9, the albumenoid matter from 0·6 to 0·28, and the chlorine as chlorides from 14·61 to 13·39. The oxygen required to oxidize the organic matter was 4·03 in respect to the sewage, compared with 1·34 for the effluent.

The disposal of the sludge is a final consideration in this as in other processes where precipitation takes place. But the sludge from the electrolytic action is not swollen by the admixture of any foreign ingredients beyond one or two grains of iron per gallon, which as oxide becomes somewhat more.

In the course of the day's proceedings Mr. Webster stated that he estimated the working cost of his process at 13s. per million gallons of sewage treated. This was irrespective of interest on capital and the allowance to be made for depreciation, except that it included the waste of iron. On 150 million gallons per day, representing the whole of the London sewage, north and south, at the present time, the cost at the

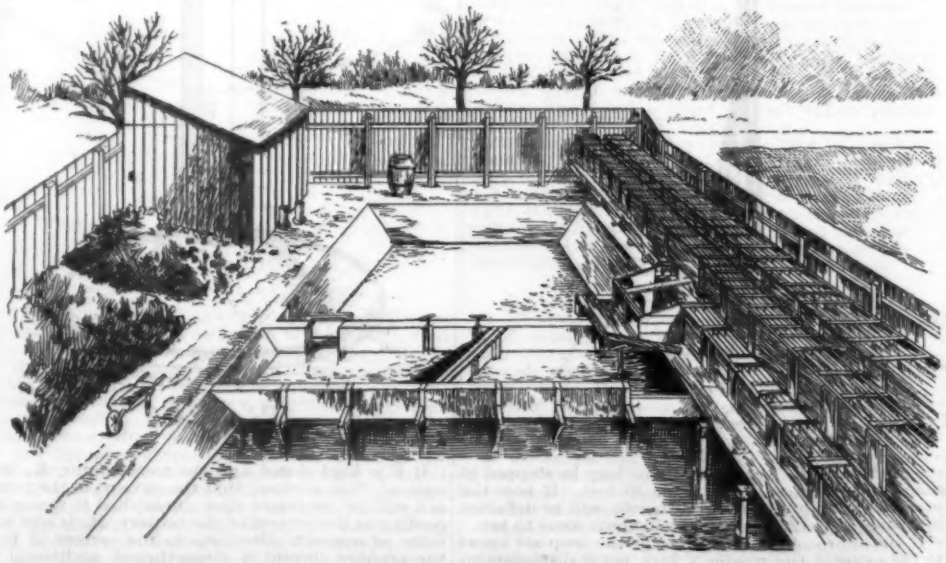
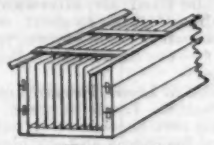


Fig. 3.—LOWER PORTION OF SEWAGE CHANNELS AND PRECIPITATING TANKS.

rate above named would amount to £36,000 per annum. Mr. Webster, in reply to various questions, said he had not undertaken any calculations as to anything beyond the working cost. He subsequently stated that the charge for interest on capital and depreciation of plant would not be likely to exceed £5,000 per annum.

Dr. Thudicum, who was present, said he was induced, from what he had seen, to look upon the process with the utmost hope. If Mr. Webster succeeded, as he trusted he would, the process would open up a new era in the history of the sewage question.



DETAIL OF SHOOT WITH IRON ELECTRODES

Fig. 4.

Sundry experiments followed, showing the action of the electric current on samples of sewage in glass vessels. In another instance, the current was made to act on water colored with an aniline dye, in order to illustrate the effect on organic matter in solution. In each case the solid particles were buoyed up by the liberated hydrogen, and collected on the surface of the liquid. On the escape of the gas the solid particles began to descend so as to form a precipitate.

We are indebted to the *Engineer* for our illustrations and the foregoing particulars.

ADDRESS BEFORE THE BOSTON MEDICAL LIBRARY ASSOCIATION.*

By OLIVER WENDELL HOLMES, M.D., LL.D., etc.

GENTLEMEN OF THE BOSTON MEDICAL LIBRARY ASSOCIATION: The books which I have offered to the association, and which you have kindly accepted, constituted my whole medical library, with the exception of a very few volumes, which, for special reasons, I retain for the present, and of a considerable collection of medical pamphlets, which I shall send if the librarian can find room for them. This collection has grown by a slow process of accretion during nearly threescore years. The first of these books which I remember owning is Bell's "Anatomy;" the last accession is the beautiful little volume which I bring with me this evening, "Elements of Pharmacy, Materia Medica, and Therapeutics," by William Whitla, M.D., physician to the Belfast Royal Hospital, fourth edition. I have not counted the number of volumes, but the librarian informs me that there are something over 900. The list of titles reaches 1,231, but the same book is in many instances given under different headings. The oldest book in the collection is a black letter copy of Avicenna, dated in 1490; the most recent, 1887; so that they cover very nearly the space of four centuries. Most of these volumes I have bought, but a considerable number have been presented to me by their authors or by friends. I trust that those of my friends who may chance to see one or more of the books they have presented me on these shelves will not think I have forgotten their kindness in allowing their gifts to follow the fortunes of the little mob of volumes in whose company they found themselves.

The most interesting parts of a miscellaneous medical library are its oldest and its newest portions. Between these is a long, dreary interval, filled up by books not old enough to be curious or even rare, and not new enough to represent the existing state of knowledge. I am thinking especially of works on practical medicine. We care nothing, or next to nothing, for the speculative notions of such writers as Forestus and Fernelius. The theories of Stahl and Van Helmont, of Hoffman and Boerhaave, the conflicts of the Brunonians and the adherents of Cullen, mean as little to the physician of the present as the farrago of remedies with which they puzzled the astonished and indignant viscera of their patients. Still, something may be gained from these obsolete and utterly neglected writers. They were not all fools because they had not the wisdom which comes with the knowledge of a later day. There were, and are always, men of common sense—good, strong, coarse common sense—in the medical profession. Sydenham was one of these. I believe I have hardly ever delivered a medical address in which I have not referred to the "roast chicken and pint of Canary" which he prescribed for a patient affected with what a Dublin physician might probably call "male hysteria." I have always thought it a stroke of genius in Dupuytren—if the idea was original with him, but I have an impression that it was borrowed from an earlier authority—when he ordered a patient to drink the milk of a goat which had been rubbed with mercurial ointment, the patient having a dread of mercury as commonly administered. That was a kind of infinitesimal dosing, and I have no doubt that the patient got as much good from the mineral in the goat's milk as the sultan did from the drugs in the handle of the bat with which he exercised every day until he was in a free perspiration. But Sydenham's prescription was the best lesson the overdrugging doctors ever got, unless it may be from watchful mothers and keen-witted, observant nurses.

As I have referred to the infinitesimal dosing business, let me remind you that, if you wish to understand the pseudo-scientific vagaries of to-day, you will find more instruction in the "Ortus Medicinæ" of Van Helmont than in any other old work with which I am acquainted. The infinitesimal or make-believe dosing is to be found in the chapter entitled "Butler." Butler was an Irishman in a Belgian jail, where Van Helmont found him. He had a pebble which he used to dip in oil, then take a teaspoonful of that oil and add it to a small flask of the same oil and apply one drop of this to the ailing part. Then he would give his pebble a lick with his tongue, and clap it in his waistcoat pocket. This whole chapter, "Butler," should be read by the student of Hahnemann and by all intelligent students of medical history. The doctrine of the "mind cure" is abundantly illustrated in other chapters of the "Ortus Medicinæ," the date of which in my copy, now yours, is 1652.

Do not look with contempt, then, on such of your old books as seem to be mere treasuries of unwisdom. The debris of broken systems and exploded dogmas form a great mound—a *Monte Testaccio* of the shards and remnants of old vessels which once held human beliefs. If you take the trouble to climb to the top of it, you will widen your horizon; and in these days of specialized knowledge, your horizon is not likely to be any too wide.

No doubt there are many rusty old volumes in this collection which will rarely, if ever, be taken from the shelves except to be spanked and dusted. Do not grudge them the room they take up. Once or twice perhaps in a score of years some curious student may open the book containing the record of cases treated by Riverius, as he called himself in Latin, a noted physician of the first half of the seventeenth century. He will see in the same pages one recipe with the cloven footed R which he uses in his own prescriptions, and another prescription with the astrological sign for Jupiter, from which it was imitated. He has hardly realized before, it may be, that in writing the letter for *recipe* as he does, he is appealing to a heathen deity to bring good luck to his remedy.

There is a formidable phalanx of grand old folios which give dignity to the lighter volumes beneath which they gravitate to the lower shelves. There are the huge volumes of Galen, with the appalling "Index" of Brassavolus; there is the solemn "Sepulchretum" of Bonetus; there are the two stately and richly dressed volumes of the "Artis Medicæ Principes," which came from the library of Jules Cloquet; there is the wonderful collection of rare and interesting cases of Schenckius; there is Ambroise Pare, a good copy in the original French, which is not always easily to be procured—my order was out some time in Paris before I could obtain it, and that was more than fifty years ago; there is Wigan's edition of Aretæus, a good specimen of what Oxford could do in the way of printing a century and a half or more ago. A little higher up in the shelves you will find Vanderlinden's Hippocrates, Cælius Aurelianus, Celsus, the works of Morgagni, the score of quartos in which Haller has left the impress of his lucid and richly stored intelligence, and many other works of earlier or later date which are not likely to be worn out by handling, but may now and then be consulted for some special purpose by a scholarly practitioner or a curious student of medical history.

More interesting than most of the old medical books are the illustrated works on anatomy. The splendid plates of Albinus show the muscles as no other work that I am acquainted with has ever shown them. The old folio edition of Vesalius must be interesting to every student of anatomy. The figures in the huge volume of Spigelius will always attract attention for the grace and beauty of the females, who display their viscera as if they were their jewels and laces. These are not likely to be overlooked by the lovers of undisguised nature and naked truth. Mascagni's representations of the lymphatics, Soemmering's pictures of the organs of sense, Bonamy and Beau's colored plates of the bones, ligaments, and muscles, and Hirschfeldt's and Leveille's illustrations of the nervous system are worthy of special notice. Among the general works which may repay occasional consultation I will only mention the entire series of the *British and Foreign Medical Review* while under the editorship of Sir John Forbes, and the "Compendium de Medecine Pratique," in eight large octavo volumes. I have spoken of a few of these books which occur to my recollection. The names of Avenburgger, Bayle, Corvisart, Andral, Louis, Chomel, Rostan, Bouillaud, are represented in a series of scientific and practical treatises, valued in their day and generation, not wholly valueless by any means at the present time.

But the practitioner of to-day who has a few manuals on his shelves, and takes one or more of the best journals, American or foreign—both if possible—can get along with a very moderate library. A working physician is very apt to come under one of the two following categories: he is either an optimist, satisfied with his old remedies and formulæ; or he is in a state of chronic discontent with his drugs and mixtures, and constantly on the look-out for something which *will* do what so many other things have promised to do and failed. New remedies keep coming up in the journals, like new shoots in asparagus beds; and they must have those that have come up last, as they want their asparagus cut fresh every morning.

Since medicine has run into specialties, as it did thousands of years ago in Egypt, there must be forming great numbers of private libraries, more or less complete on their particular subjects. We may hope that in time all the important branches of medical science and practice will be fully represented on our shelves. But the enormous amount of material already accumulated in many special departments would be of comparatively little use without indexes to point out the way to find what is wanted. The great "Index Medicus" prepared at Washington, under the direction of the admirable Dr. Billings, is the master key to the storehouses of knowledge, which without it few would ever have even tried to open.

These books were very dear to me as they stood upon my shelves. A twig from some one of my nerves (as I remember saying long ago) ran to every one of them. From the time when I first opened Bell's "Anatomy" to that in which I closed my Sharpey and Quain and my Braithwaite's "Retrospect" they marked the progress of my studies, and stood before me as the stepping stones of my professional life. I am pleased that they can be kept together, at least for the present; and if any of them can be to others what many of them have been to me, I am glad to part with them, even though it costs me a little heartache to take leave of such old and beloved companions.

* Delivered to the Association at the formal presentation of Dr. Holmes' Medical Library, Jan. 29, 1889.

ACTION OF SANTONIN ON THE NERVES OF THE EYE.

AT a recent meeting of the Physiological Society, Dr. A. Konig gave an account of his experiments on the action of santonate of soda on the perception of colors. As is well known, a distinction is drawn between congenital and acquired anomalies of the color sense (color blindness); of these the first only gives rise to color blindness to red or green, while color blindness to violet is never observed as a congenital defect. On the other hand it was supposed that, in the anomalous perception of color which results from the action of santonin or santonate of soda, we had to deal with a typical case of acquired color blindness to violet. The speaker had hence been led to make a number of experiments with santonate of soda on himself, and, apart from the fact that as soon as its action is manifest all objects appear of a yellow color, had established the following phenomena. The spectrum ceases to be visible on the hinder side of the blue, and not a trace of violet is ever visible. The neutral point, as deduced from closely agreeing measurements, is situated at wave length 573—that is to say, exactly at that point which is complementary to the missing violet. The speaker based upon these observations the conclusion that the visual phenomena which are observed after the administration of santonin are not really of the nature of color blindness to violet, but can be completely explained by the assumption that the violet rays are absorbed by those media of the eye which have been affected by the drug. Prof. Preyer was unable to agree with the above conclusion, speaking with the experience of the experiments he had himself made with santonate of soda in 1868. The fact that after the administration of the drug the violet part of the spectrum can be seen when it is looked at, not directly but indirectly, is opposed to Dr. Konig's views—that is to say, when its image is allowed to fall upon peripheral parts of the retina. Moreover, Prof. Preyer stated that he experienced a distinct sensation of violet when he had taken the drug while his eyes were closed, and then opened them after the action of the drug had become manifest. He believes that the visual phenomena which accompany the action of santonin can only be explained by assuming that it affects the central nervous system, and that this view is supported by the abnormal gustatory, olfactory, and auditory sensations which are simultaneously observed.

[FROM THE ASCLEPIAD.]

JOSEPH PRIESTLEY, LL.D., F.R.S., AND THE DISCOVERY OF VITAL AIR—OXYGEN GAS.

By BENJAMIN WARD RICHARDSON, M.D., F.R.S.

IN the strict sense of the word, Joseph Priestley did not belong to the brotherhood of medicine. In a general sense he did, for he belonged to all brotherhoods of learned men. Also to no order of learned men did he render more important service than to our order, for he unlocked one of the secret recesses that lead to the mystery of life. There needs no apology, therefore, for placing Priestley among our great physiologists and philosophers, whose life and works no son of Esculapius can study without being wiser and better for the labor.

Joseph Priestley was born at Fieldhead, near to Leeds, on the 13th of March (Old Style), 1733. He was the firstborn of his parents, people of very simple and industrious lives. His father, Jonas Priestley, was a dresser of woolen cloth, as was Joseph Priestley, the father of Jonas. His mother was the only child of Joseph Swift, a farmer at Shafton, a village six miles southwest of Wakefield.

Five other children were born to Jonas and Mrs. Priestley, three sons and two daughters, the mother of them dying when our Joseph was about seven years old. Three years later his father married again, and by his second wife had three daughters. He could remember but little of his mother, but was able to recollect that she was careful to teach him the Assembly's Catechism, and to give him the best instruction during the little time he was with her. Once, in particular, when he was playing with a pin, she asked him where he got it; and on his telling her that he found it at his uncle's, where he had been playing with his cousins, she made him carry it back again, the place being near, in order to impress on his mind, as she did, a clear idea of the rights of property and of the importance of attending to it. "She died," he says, "in the hard winter of 1739, not long after being delivered of my youngest brother; and having dreamed, a little before her death, that she was in a delightful place, which she described and imagined to be heaven, the last words which she spoke were, 'Let me go to that fine place.'"

After the loss of his mother Priestley lived for a time with a maternal uncle, but was removed from him in order that his brothers and sisters might be allowed, in turn, to share in the same advantage; and, his father being encumbered with a large family, he was sent to school near to his home. He did not remain in this position long, for a sister of his father, Mrs. Keighley, who was childless, took a liking for him and became to him a second mother. Soon afterward Mr. Keighley died, and being a man of considerable property, all of which he left to his wife, the accepted, if not adopted, son found in the house of his aunt a home from 1742 until he entered his academic career.

STUDENT LIFE.

Mrs. Keighley encouraged her nephew to enter the Christian ministry, to which vocation he was himself greatly inclined. Unfortunately his health showed signs of giving way, and he was induced to try some pursuit that should not severely tax his mental powers. He must, he thought, go into trade, and having great facility for learning languages, rapidly taught himself French, Italian, and High Dutch, in the first and last of which he translated and wrote letters for one of his uncles who was a merchant, and who intended to place him in a house at Lisbon. Fortunately for learning and for science, although a house was actually engaged to receive him in Lisbon, and everything was ready for his voyage, his health improved, and his old determination to enter the ministry returned. He was therefore sent to Daventry to study under Mr. Ashworth, afterward the widely known Dr. Ashworth.

It is worthy of remark in this part of the early life of Priestley, that in the interval between his leaving school and entering Dr. Ashworth's academy at Daventry, he began to acquire a taste for mathematical and other scientific studies. Mr. Haggerstone, a dissenting minister in the neighborhood, taught him geometry, algebra, and various branches of mathematics, theoretical and practical; he read also Graves-

end's Elements of Natural Philosophy, Watt's Logic, Locke's Essay on the Human Understanding, and other classical works. In these studies he made such proficiency that when he joined the academy, in the year 1752, he was excused all the exercises of the first year and a great part of those of the second.

In following Priestley through his academical course two incidents which tended greatly to shape his after life require to be recorded. In going to hear any discourse he got into the habit of committing the heads of it to paper. By this means he rested his memory. He did more; he learned to compose in a rapid and lucid style, a gift which proved to be of the utmost service to him in all his subsequent work.

The second incident to which it is necessary to refer relates to a change of mind and an emancipation from early dogmas, which made him a new man in the midst of men. A personal matter led to the change named. In his desire to jot down quickly what he heard or thought, he determined to learn shorthand, and for that purpose purchased Annet's book on that subject. Thinking he could suggest some improvements in shorthand, he wrote to the author, and in that way established a correspondence which lasted several years. Annet was a man of free thought, and an advocate of the Necessarian doctrine, on which latter topic he and his pupil corresponded freely. At the beginning of the controversy Priestley defended philosophical liberty against the Necessarian school; but in due course he, too, became a convert to Necessarianism, and at the close of his life declared that "he had derived the greatest advantage from the full persuasion of the truth of that doctrine."

Let those who wonder at the conversion of Joseph Priestley to these views think the matter out, and the wonder itself will pass into the rule of necessity. In plain terms, Priestley in becoming a philosopher became also what he was bound to become, a philosophical Calvinist. As a child he had been taught that everything is preordained; as a man he learned that in nature everything goes by fixed laws, so that nature and Calvinism—if it be not insult to nature to link her name with John Calvin—are one and the same.

Under Dr. Ashworth, Priestley remained three years, from 1752 to 1755, and during the whole time, he tells us, his days passed "with that peculiar satisfaction with which young persons of generous minds usually go through a course of liberal study in the society of others in the same pursuits, and free from the cares and anxieties which seldom fail to lay hold of them when they come out into the world."

In the course of his studies in the academy, Priestley began to read Hartley's "Observations on Man," a book which produced the greatest and, as he believed, the most favorable effect on his general turn of mind, throughout the whole of his life. It established in him the belief of necessity; it greatly improved his disposition to piety, and it freed him from the rigor with which his piety had been tinctured. Step by step he moved out of the pale of strict orthodoxy into the pale of ancient Arianism, and such was the spread of this phase of faith in and among his fellow students, that the whole of them were more or less influenced by it.

These were the courses by which our aspirant for the ministry prepared his way for that vocation, and by which he became, in the end, one of the sect of "Sociniana," the sect better known, with some modification of doctrine, as "Unitarian." He was already a student of natural as well as of orthodox religion; already he had conspired with himself to live by sight as well as by faith, and had composed a copy of "Institutes of Natural and Revealed Religion."

An exceedingly characteristic trait belonging to Priestley was developed in this period of his career. He suffered from "a thorn in the flesh." He had a family failure of speech, the failure of stuttering, or stammering. It was a cause of much distress to him. "However," he adds, "I hope it has not been without its use. Without some such check as this I might have been disputatious in company, or might have been seduced by the love of popular applause as a preacher: whereas my conversation and my delivery in the pulpit having nothing in them that was particularly striking, I hope I have been more attentive to qualifications of a superior kind."

FIRST MINISTRY.

With this acknowledged inferiority in pronunciation, and with a fearful heresy in his breast, the young philosopher entered on his first ministerial charge at Needham Market in Suffolk. He delivered his first sermon to a small congregation of about one hundred people. Like Goldsmith's vicar, his stipend was only about forty pounds a year, and yet for half a year or so everything seemed promising. Alas! he ventured too far; he opened a series of lectures on the theory of religion; several of the audience began to doubt his orthodoxy; his congregation began to fall off apace, and, although some of the best families remained with him, his salary came short of thirty pounds per annum; and had it not been for Dr. Benson and Dr. Kippis, who now and then got for him five pounds from different charities, he could not have been sustained. Added to these misfortunes came another, namely, the absence of remittances from his aunt, who, partly owing to the objection she had taken to his new religious views, and partly to the fact that she had expended her income more freely on other objects, had drawn together the strings of her purse in so far as he was concerned. Finally, with his entire assent, she left the whole of her property to a niece who was deformed, and who was most in need of the bequest.

But what contributed greatly to his distress was the impediment in his speech, which increased so much that relief was necessary. He went, therefore, for the first time to London, with twenty guineas supplied by his aunt, in order to go under the treatment of one Mr. Angier, a "specialist" in the treatment of stammering. Angier attended him about a month, making him take an oath not to reveal his method. He received some benefit, but soon relapsed, and became worse than ever.

On returning to Needham Market, Priestley began to feel the effect of a low, despised situation, together with that arising from the want of popular talents. He worked laboriously, and his life at this place was monotonous enough; but it is remarkable as being connected with another development of thought which led him ultimately into science and to the final grandeur of his brilliant career.

THE TOUCH OF SCIENCE.

In order to make both ends meet, he commenced to give lessons in natural science to about ten pupils, to whom he taught the use of the globes and the simple elements of natural philosophy.

After a weary sojourn of three years in this ministry, Priestley, through the friendship of Mr. Gill, a maternal relative, was invited to preach as a candidate at Sheffield. He was not approved, but Mr. Haynes, another minister, recommended him to a congregation at Nantwich, in Cheshire. He accepted the post in 1758, and going first to London, went from thence to Nantwich by sea, to save expense.

At Nantwich he found a good-natured, friendly people, with whom he lived three years very happily, and among whom he established a school of thirty boys, with a separate room for some half dozen young ladies; he also gave lessons out of doors, and yet found time to learn to play on the flute and to compose a new grammar of the English language. He seems to have prospered also in a worldly point of view, but receiving an invitation to join an academy at Warrington as "tutor in the learned languages," he migrated there, and applied himself, with all the honesty and energy of his character, to his new duties.

While at Warrington the greatest event of his life happened to Priestley; he fell in love with Miss Wilkinson, the daughter of Mr. Isaac Wilkinson, an ironmaster of Wrexham, and sister of one of his pupils. His suit was successful, and he was so fortunate as to marry one who was, he says, "a woman of excellent understanding, much improved by reading, of great fortitude and strength of mind, of a temper in the highest degree affectionate and generous, feeling strongly for others and little for herself." She was also a good housewife, and relieving him of all household cares, allowed him to devote all his time to his studies and to the other duties of his station. At Warrington he had born to him a daughter, Sarah, who afterward became Mrs. William Finch, of Dudley.

The taste for teaching science inaugurated at Needham Market was continued at Warrington, and led to the delivery of a course of lectures on anatomy. Once a year also he went to London in order to gather up new information and to make new friends and acquaintances, in which he was so happy that he added to his list of friends Dr. Price, Mr. Canton, and the famous Benjamin Franklin.

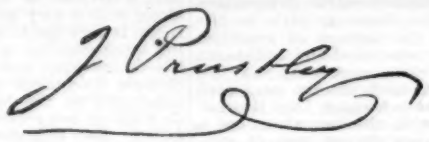
At this point in the career of Dr. Priestley we come to the period when, in the true sense of the word, he may be ranked as a man of science. Gaining a close acquaintance with Dr. Benjamin Franklin, he conversed with that most remarkable man on the question whether or not he should tackle a history of electricity. Franklin received the suggestion favorably; the great work—still a great work—was commenced, and by steady perseverance was brought to a close so quickly that, although it was perfect in all its parts and quite unique in its compendiousness, it was judged by the critics, who could not have touched the work themselves, as too hastily done. During this same period he also drew up a chart of biography, after which the University of Edinburgh conferred on him the title of LL.D., and the Royal Society, at the instance of Franklin, Watson, Canton, and Price, enrolled him as a fellow.

In 1767 Priestley removed from Warrington to take charge of the congregation of Mill Hill Chapel, Leeds, where he spent six of the happiest years of his life, "finding scope for every kind of exertion," and ever considering "the office of a Christian minister as the most honorable of any upon earth."

INTRODUCTION OF PNEUMATIC CHEMISTRY.

Nothing foreign to his duties as a minister occupied his attention at Leeds so much as his experiments in *electricity*, and especially "the doctrine of *air*." He was led to study this last named subject in consequence of inhabiting a house adjoining a public brewery, where he first amused himself with experimenting on the fixed air which he found ready made in the process of fermentation. These experiments, performed at first for amusement, absorbed his attention so much that when he removed to another house he commenced to manufacture the fixed air, afterward called carbonic acid gas, for himself; and, in short, was led to the discovery of what soon became known, from him, as *pneumatic chemistry*—the liberation, collection, and examination of gases.

While residing in Leeds, Priestley made the acquaintance of the famous Leeds surgeon, Hey, known so widely since his own time down to ours by the surgical saw that bears his name and by many excellent memoirs on surgical subjects. Hey in the religious world was a zealous Methodist, and was given to attack his Socinian friend sharply in public controversy; but on matters of science they were on excellent terms, and the zealous surgeon was ranked by Priestley as one of the few men in Leeds who took a real interest in his experiments. When Priestley left Leeds, Hey begged of him the earthen trough which had been used in the first researches in pneumatic chemistry. It was a common trough, says Priestley, "such an one as is commonly used for washing linen;" but Hey knew the important part it had played, and prized it as a rare present.



During this portion of the life of Priestley the great navigator, Captain Cook, was starting forth on his second voyage of discovery, and it was proposed that Priestley should accompany him as the naturalist of the expedition. To this he assented, and as the members of his congregation were willing to appoint a substitute for him during his absence, all seemed likely to go well. But certain clergymen on the Board of Longitude took alarm at the news of his religious opinions, and had influence enough to get his appointment set aside and to have Dr. Forster put in the place. With characteristic candor and simplicity our philosopher resigned himself to his fate—what could a Necessarian do less?—impressed that Forster was far more fitted for the post than he was, and went on with his own congenial studies and pursuits as if nothing had intervened to turn his thoughts from the natural course in which they were accustomed to flow. At Leeds two sons, Joseph and William, were born to Priestley, and altogether he was very happy there; but his income was only one hundred pounds a year, with a house which was not large enough for his increasing family. He therefore, on the recommendation of Dr. Price, left his ministry, after holding it for six years, in order to become, nominally, librarian to Lord Shelburne—later on the Marquis of Lansdowne—with a salary of two hundred and fifty pounds a year, a house to live in, and a certainty for life, on the death of the earl or of separation from him, of a pension of one hundred and fifty pounds per annum.

It is curious to trace the influence of this change of life on the philosophic and scientific minister. In some respects it was good, in others bad. It gave him freedom to pursue his researches, for the librarian work was of little moment, and by carrying him abroad it widened the sphere of his knowledge and acquaintanceship. It also gave him a kind of independence for life. On the other hand, the position exposed him to the whips and scorns of patient merit. The plain truth

seems to be that the earl, proud of the acquisition of a librarian and dependant who was a figure of true mark in the world of science and letters, was at first all smiles and kindness, then, cooling down, became courteous, in a later stage cold and polite, and at last, tired of his poor servant, abruptly ready to turn him off and see him no more, not even extending to him the courtesy of a reception to a friendly call, and not indeed knowing that there was such a man as Joseph Priestley until some new service was wanted of him. The old lines of insolent pride and wretched poverty of wealth.

The Priestley family moved from Leeds to Calne in Wiltshire, to be near to Bowood, where the earl resided in the summer; and, sometimes in Wiltshire, sometimes in London, sometimes abroad, Priestley remained with the earl for seven years. In the second year, in company with the earl, he made the tour of Flanders, Holland, and Germany, as far as Strasburg, spending a month finally in Paris. In Paris he met the famous Magellan, with whom he spent delightful days, and with whom he returned to England.

NEW SCIENCE WORK.

We have arrived now at a time when Joseph Priestley was in his forty-second year, and, still in days of favor with the master he was nominally serving, was free to carry out some researches which had occupied his thoughts at Warrington. He resumed his experimental pursuits, and was encouraged in his work by Lord Shelburne, who liking to see him perform his experiments "to entertain his guests, especially foreigners," allowed him a sum of forty pounds a year for the simple apparatus required for the great service to science performed at the laboratory in Calne. This is the time when we, as medical men, are more than any other interested in the work performed, since it revolutionized our science beyond anything that had been accomplished from the time William Harvey disclosed the secret of the circulation of the blood.

The grand discovery that came to the world from Calne, in Wiltshire, by the labors of Priestley was that of *vital air*, or, as we now call it, *oxygen gas*. We must be just in thinking of this discovery, and of the part which our present great man, its true discoverer, took in it. We must be as fair as he would be himself, and if we are that, we shall be fair enough, for never in the history of original men was there one fairer or juster than he in accepting and acknowledging the claims and labors of his predecessors and of his fellow-workers.

It had long been believed, before the time of Priestley, that there exists in the air some vital spirit or principle which is absorbed by living animals in the act of breathing, and which, removed from a confined chamber of air, reduces the quantity of air, as if something were lost from the air. In the life of John Mayow (*Asclepiad*, Volume IV., pp. 56–70) we saw how that neglected genius anticipated the discovery of the vital spirit of the air, and how he actually forecast the discovery of the existence of that vital spirit as present in saltpeter—nitrate of potassa. But up to Priestley nothing had been demonstratively done to separate from the air, or from any other substance, this spirit of the air. Certain phenomena had led to the inference that such a thing must exist, just as in our own time certain external phenomena led astronomers to infer that a planet existed the other side of Uranus; but the thing itself was not revealed to the senses. Priestley revealed it, but he had first to overcome a prejudice that the atmospheric air is anything more than air combined with an inflammable something, called, before his day, phlogiston.

The mode in which the discovery was made was simple of the simple. Its author is particular to nicety in impressing this fact on the attention of his readers. He says that "the record of his discovery furnishes a striking illustration of the truth, which can hardly be too often repeated, that in philosophical investigations more is owing to what is called chance—that is, philosophically speaking, to the observation of events arising from unknown causes—than to any proper design or preconceived theory in the business."

How Priestley got at vital air is clearly told by him in his essay "On Dephlogisticated Air and the Constitution of the Atmosphere," in the second volume "Of Experiments and Observations on Different Kinds of Air," published in 1775, one year after the discovery. He says that after he had satisfied himself that the air "was alterable," he came to the conclusion that it was not an elementary substance, but a composition. Then he began to think "what this composition could be, or what is the thing that we breathe, and how is it to be made from its constituent principles?"

Previously to this time he had been extracting air from a great variety of substances by means of the heat obtained by a burning glass. His plan was to put the substance he wished to decompose into a conical-shaped glass vessel, and then to fill the vessel with mercury. The vessel was now inverted over mercury, by which means the powder to be decomposed was brought to the inside surface of the glass, between the glass and the mercury, in such a manner that the heat of a burning glass could be made to play upon it. If under the influence of the heat any gas were liberated, it displaced the mercury and was retained in the vessel ready for examination.

DISCOVERY OF VITAL AIR—OXYGEN.

With this apparatus Priestley discovered at Calne what we call oxygen, on the 1st of August, 1774. "I endeavored," he says, "to extract air from *mercurius calcinatus, per se*, and I presently found, by means of this lens"—a lens twelve inches in diameter and twenty inches focal distance—"air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found that it was not imbibed by it. But what surprised me more than I can express was that a candle burned in the air with a remarkably vigorous flame, very much like that enlarged flame with which a candle burns in nitrous air, exposed to iron or lead sulphur; but as I got nothing like this remarkable appearance from any kind of air besides this remarkable particular modification of nitrous air, and I knew no nitrous acid was used in the preparation of *mercurius calcinatus*, I was utterly at a loss how to account for it."

In the above lines we get the history of the discovery of oxygen, but we are not at the end of the original chapter. From the *mercurius calcinatus* the discoverer moved to *common red precipitate*, a substance obtained by acting on a solution of mercury with spirit of niter. From this red precipitate he again extracted the remarkable air, by the heat of his burning glass, and feeling sure that the liberated spirit or air had been communicated to the mercury by the nitrous acid, he drew, for the first time, the inference that as *mercurius calcinatus* is produced by calcining mercury in common air, therefore mercury in the act of calcination *takes something from the air*, which something is, again, yielded up when it is subjected to heat.

At the same time that he extracted the air from the *mercurius calcinatus* he liberated it also from *red lead* or *minium*, and he observed in this process that part of the red lead on which the focus of the lens had fallen turned yellow. One-third of the air in this experiment was readily absorbed by water, but in the remainder a candle burned very strongly and with a crackling noise. This experiment with red lead confirmed him in the idea that the *mercurius calcinatus* must get the property of yielding the new kind of air from the atmosphere, the process by which red lead is made being that of calcination.

From November, 1774, to March, 1775, Priestley continued his researches on his new product without fully realizing the treasure that was in his hands. He found that, as a gas or air, it was not very soluble in water; he found it would keep so purely that after several weeks a candle would still burn in it with undiminished splendor. And now he made a new experiment with his treasure, which opened his eyes still further as to the composition of the atmosphere that surrounds our planet, and the nature of its vital services.

He had observed in some previous researches that when nitrous air, got by acting on copper with nitrous acid, and now called nitrogen dioxide, was exposed to common air it became a gas or fume of deep red color, readily absorbable by water, to which it imparted an acid reaction. The fact led him to try what would be the effect of exposing his new gas or spirit to the same test. He put one measure of nitrous air to one measure of the new air, and found not only that the mixture of the two gases was freely absorbed by the water, but that the red fumes were produced just in the same way as when the nitrous air was mixed with atmospheric air, after which he had no doubt that the air from *mercurius calcinatus* was fit for respiration, and had all the other properties of genuine common air.

The line of experiment carried so far must needs lead to something more of a vital character. Would the newly discovered air, in which a taper would burn so brightly, support also the animal fire?

On the 8th of March, 1775, a living mouse was put into a glass vessel containing two ounce measures of the air from *mercurius calcinatus*. Had common air been used, a full grown mouse, as this was, would have lived about a quarter of an hour. In this air "my mouse," says Priestley, "lived a full half-hour, and though it was taken out seemingly dead, it appeared to have been only exceedingly chilled, for upon being held to the fire it presently revived, and appeared not to have received any harm from the experiment." Later on another mouse was subjected to the same experiment, and lived in the gas for three-quarters of an hour.

This was the discovery of vital air, a discovery of first rank in science, and truly vital in its import. That it was enveloped at first in a web of what now appear to be childish difficulties was no more than might have been expected, for nitrogen had still to be separated as a constituent part of the common atmosphere; but the discovery of vital air led the way to this separation, and "the nitro-aerial spirit" of John Mayow was revealed practically to mankind.

MEDICAL AND HYGIENIC DISCOVERIES.

As a gift to physiological medicine we may look upon this discovery of Priestley's as immortal. But one or two more discoveries, each of which would alone have made any man famous in science, require our regard.

The introduction by him of the pneumatic method of investigation constituted in itself an event in the history of scientific research. To this must be added the discovery of nitrous oxide gas, from which, under the genius of Humphry Davy, the grand development of modern anæsthesia afterward took root. To these must also be added the plan of charging water with gases and the entire introduction of effervescing medicines and aerated waters.

And yet another revelation, not precisely medical, yet distinctly hygienic in its bearings, comes to us from his masterly mind. I refer to the discovery of that interchange of gases which takes place between animals and plants. He detected that plants by their leaves absorb or take up the fixed air or carbonic acid which is evolved from the slow-burning bodies of living animals or of quick-burning dead substances like coal; that they decompose this fixed air, utilize the carbonaceous part of it, and give back the vital air to the animal world. Plants, therefore, he argued, are the grand purifiers of the air and maintainers of the balance of life between the animal and vegetable kingdoms.

Other works of science contributed by Joseph Priestley to science, his labors on electricity, on vision, and on colors, I have not space here to describe. Neither have I the opportunity of so much as touching on his wonderful theological works, which, in the presence of his scientific, have been wofully neglected. It is necessary to close the narrative by returning to the life of the illustrious scholar, and by tracing it to its perplexed yet, to him, "born of a happy disposition," as he says of himself, happy termination.

RETURN TO THE MINISTRY.

I have stated, by anticipation, that after a few years—seven, in fact—Dr. Priestley left Lord Shelburne. Nothing else could be expected. Reflecting afterward on the time he spent with Lord Shelburne as "the guest of his family," he was not, he could truly say, "fascinated with that mode of life." He was not unhappy, much less so than those who were born to the state, but he was happier when he got back to a sphere where there was a sufficient motive for exertion on other objects beyond amusement, more happiness, more virtue, more true politeness, and less necessity for disguising passions and feelings which must be concealed, but not without an effort as difficult as it is severe.

For some time before leaving Lord Shelburne, Priestley found that, without any apparent cause, there was some dissatisfaction. He asked the earl if he had any fault to find with him, and the answer was, *None*. The simple fact was that the great man became tired of his guest, and so, through Dr. Price, he offered him an establishment in Ireland, which was of course declined; and on this they parted amicably.

From Calne, where he had another son, named Henry, born to him, Priestley returned to London about the latter part of 1778. Here he found many friends who helped him generously on his way. Mr. and Mrs. Lindsey, Mrs. Rayner, Mr. Lee, Dr. Fothergill, Dr. Heberden, the Messrs. Galton, Mr. Wedgwood, and many others came gladly to his aid. But above all his other friends, as related to science, was the illustrious Benjamin Franklin. His account of their intimacy is most touching. They spent together the last day of Franklin's sojourn on English soil.

At the suggestion of his brother-in-law, Mr. John Wilkinson, Dr. Priestley, about the year 1780, settled down in Birmingham, where Mr. Wilkinson himself resided. Here, after a three months' stay, he joined an Independent minister, Mr. Blyth, in charge of one of the free congregations of that time, a position which was in every sense congenial to him, and which permitted him to pursue his scientific inquiries with the greatest facility among such friends as Boulton, Erasmus Darwin, Withering, and the immortal James Watt. These philosophers formed, at the time under consideration, a learned society, which they called the *Lunar Society*, because it was held at the period of full moon. Watt's "iron horse" was in course of development, and was truly in the womb of time, but it was not old enough to work a locomotive, so the friends, who lived long distances apart, were obliged to take advantage of the full moon in order to see their road home after their meetings. Once in my life, being at Leamington, I had the honor of breakfasting with one who had taken part in these gatherings, Mrs. Galton, the daughter of Dr. Erasmus Darwin. She recounted to me her vivid remembrance of a Lunar Society meeting at her father's house at Derby. She depicted herself as a young maiden receiving the visitors; she remembered her father coming up the garden to meet them, bearing a rose; she described making tea for them before their meeting; and she also remembered seeing them depart after supper, and hearing a friendly altercation between Priestley and Watt as to who should drive the horse that drew the gig in which they were seated to take their long night journey homeward.

DAYS OF STORM.

Life went on smoothly with our philosopher at Birmingham until 1789, when the question about the "Test Act" was much agitated in and out of Parliament. In the excitement the Dissenters in general, throughout the kingdom, became unpopular with the Court; and, the established clergy being very active and bitter, the commotion grew hot and troublesome. The temper of the time was hard on the plain-spoken and honest Priestley. Mr. Madan, a clergyman in Birmingham, preached at him, and on the occasion of the celebration there of the anniversary of the French revolution (July 14, 1791) by some of his friends, but with which he, Priestley, had very little to do, a mob, encouraged by persons in power, first burned the meeting house in which he preached, then another meeting house in the town, and then his dwelling house, demolishing his library, apparatus, and, as far as they could, everything belonging to him. As I pen these lines there lies before me a fine engraving of this disgraceful scene of desolation, from a picture once in the possession of Mr. Joseph Parker, and presented to me by the great-grandchild of Priestley, my good friend Madame Belloc, *nee* Bessie Parkes, poetess, and first editor of the *Englishwoman's Journal*. The picture, Hogarthian in its treatment, and sketched on the spot, is a fearful scene of drunkenness, ignorance, bigotry, and misery.

That Priestley himself was in personal danger of his life is stated by himself, and I have been told by one who recollected the events perfectly that he would certainly have been killed had he been found at home when the mob reached his house. Fortunately he was from home, and, according to my informant, was saved by mounting a horse and riding on the road toward Worcester, the fact of his being on horseback, and some slight disguise, preventing him from recognition until he was out of danger. He returned to Birmingham no more to live, but removed to London as the safest place of abode until greater quiet prevailed. In London he was received by Mr. William Vaughan, and was soon afterward invited by Dr. Price's congregation, at Hackney, to succeed, as their minister, that eminent man, who had recently died; a charge he accepted, together with another of a professional kind in New College.

Good and true friends stood by Priestley at Hackney, and he had no wants that were not supplied. But in what were called the higher circles he was, as we should say in these days, a boycotted individual. Even the Fellows of the Royal Society, a society he had so largely honored by his labors, "shunned" him, so that at length he was obliged to withdraw himself from them. It added to his misfortunes that when the national convention of France was formed he was invited, by many departments, to be a member of it, the honor of which he declined, but the mischief of which remained, his name being combined with that of Mr. Thomas Paine, who not only was invited, but who accepted the position, and but for the providential oversight of a gaoler, would have lost his head as the result. To what extent the antagonistic feeling influenced some of the persecutors of our scholar is shown by a doggerel which I once met with:

> "All infidels and Jacobites
> With old Tom Paine may go,
> Along with Dr. Priestley,
> To Beelzebub below."

It is too painful to read the narrative Priestley has left us of the persecution to which he continued to be subjected. At the assizes at Warwick he was shamefully used. His eldest son, in business at Manchester, was forced out of partnership because of him. His neighbors in Clapton, where he had taken a house and started a laboratory, were seized with alarm lest his house should again be burned down; and he was burned in effigy. The government would afford him no protection, and Mr. Burke had the mean audacity to declare of him in the House of Commons that he was made a citizen of France on account of his de-

cleared hostility to the constitution of this country, a statement Burke had "neither the ability to maintain nor the virtue to retract."

At length, under these persistent and cruel injuries and insults, only one remaining course was left open to this illustrious discoverer and benefactor of our country. Surely no darker blot stains our annals than the treatment of one of England's truest, noblest, most patient, and most faithful children of light.

IN EXILE AND DEATH.

The sons of Priestley, finding no rest in their own country, sailed for America, and in the little settlement of Northumberland in Pennsylvania, at the head of the Susquehanna River, found a new home for their father and mother, to which both retired, and from which, on March 24, in the sixty-second year of his age, our still happy philosopher completed a sketch of his eventful life. He left England on April 8, 1794, and arrived at New York on June 4. Received very kindly at New York, he passed to Philadelphia, where he was presented with an address from the American Philosophical Society, and was unanimously chosen Professor of Chemistry of the University of Philadelphia, a post he felt he could not accept. In the middle of July he reached Northumberland, with a fixed determination to move no more. Here, therefore, a house was built for him with a laboratory, and here he continued his favorite studies in theology and his experiments in the field of science. In the morning he rose with the sun, lighted his own fire, and with slight interruptions for meals, continued steadily at his work for a full period of six hours. The work finished, he rambled into the surrounding beautiful country, all new and varied to him, and, refreshed with the exercise, returned home ready to play with his grandchildren, or take part in some friendly game of chess or cards with his beloved wife or friends, until bedtime, an early hour with him. He was a good sleeper, "falling asleep as soon as he became warm," and waking early next morning was ready to go through a day of work or care with any one of his compeers. Like Walter Raleigh, he was very fond of the garden, and, like most great men, he loved music, although, as a performer, he never got beyond the flute of which we have heard, and not far even on that despised instrument.

The life of Joseph Priestley was all gentleness. His face, as seen in the portrait—for the copy of which I am indebted to one of his honored name and family, my old friend Dr. W. Overend Priestley—shows this fact better than any one can tell it; so also does his correspondence. A letter, lent also by my friend, from which the autograph at the foot of the portrait is engraved, tells the same story; it is to his sister about a relative who required aid, a letter graceful and tender, the very beauty of holiness. His address on leaving England is in similar tone, but loftier and more refined. In fact, there is only one passage in the history of all humanity that, in my opinion, approaches it, which passage I must leave the reader to divine, lest I may be thought irreverent in simile.

In the early part of the year 1803 Joseph Priestley, failing in vital power, began, for the first time in his life, to give up his usual occupations. Toward the end of January he lost, for a moment, the power of speech, but "never felt more pleasantly in all his life." He continued to fail, and was quite conscious of the fact. A friend calling on him, he observed, "You see, sir, I am still living." "You will always live," replied the friend. "Yes," he responded, "I believe I shall, and we shall all meet again in another and a better world." On Sunday evening, February 5, he had his grandchildren by his bedside, wished them good night, and told them he was going to sleep as they were, for death was only a long sleep. At four in the morning of the 6th February he called his son Joseph, took from him some refreshment, and then, reposing until ten o'clock, got him and Mr. Cooper to bring three publications which they had looked out for him. On these he dictated alterations, and, when the task was over, and the new matter was read, he said, "That is right, I have now done." About an hour after, at his request, he was moved from his bed to a cot, and ten minutes later ceased to breathe, so easily that his son and his son's wife did not know he was dead. "He had put his hand to his face, which prevented them observing it."

My good friend, the late Mrs. Joseph Parkes, granddaughter of Joseph Priestley, who often conversed with me about him, who, knowing my deep interest in his life and work, was never wearied in answering my inquiries, and who supplied me with much information that has been of service in writing this short history, was one of the children to whom the illustrious man said the last good night. She was then six years of age, and she told me that on the morning of his death, knowing nothing of the great event, she took her stool and her book, and going to the side of the cot on which he lay, sat down to read, thinking, from the placidity of his features, that he was simply in a gentle and natural sleep. She sat there, reading her lesson, as she had often done, in perfect happiness; when, to her surprise, her parents, finding her and gently removing her, explained to her what she could not at first believe or realize. Perchance it was the recognition of this or some similar scene that led another friend of the past, the late Sir John Bowring, to write the best verse of one of his exquisite hymns, which so fitly closes my present theme:

"So through the ocean tide of years
The memory of the just appears;
So through the tempest and the gloom
The good man's virtues light the tomb."

[NATURE.]

ON THE LIMIT OF THE SOLAR SPECTRUM, THE BLUE OF THE SKY, AND THE FLUORESCENCE OF OZONE.

THERE are two facts of particular interest which have been observed in connection with the light which we receive from the sun and the sky. First, though the ultra-violet spectrum of the sun is very well represented by the iron spectrum taken from the electric arc, yet its length is nothing like so great, and there is no fading away of feeble lines and a weakening of strong ones, which would be the case if the rays were affected by a turbid medium through which they were transmitted, but there is a sudden and sharp extinction, which points to a very definite absorption. Secondly, light from the clearest sky, unaffected by aqueous vapor, is of a deep and beautiful blue color, more of an indigo-violet tint than ordinary so-called sky-blue. There is nothing more beautiful in nature than the blueness of the heavens.

The limitation of the solar spectrum has been the subject of elaborate investigation by M. Cornu.* He proved by direct experiment that the ultra-violet rays are absorbed with energy by the atmosphere, and showed that there is a variation in the amount of absorption corresponding with different altitudes, so that the absorbent matter is at each elevation proportional to the barometric pressure, and consequently in constant relation to the mass of the atmosphere. This fact alone is sufficient to exclude water vapor from consideration as being the medium of absorption. Moreover, water vapor, while it absorbs the red and infra-red rays, transmits the ultra-violet very completely.

Photographs taken in 1879 on the Riffelberg, at an altitude therefore of 8,414 feet, reached to wave length 2,932; but at Viege, an altitude of 2,165 feet, to only 2,954.

In short, it was shown that within the limits of altitude at which Cornu's observations were made there was a difference of 10 tenth-metrets of wave length for every 984 feet of dry atmosphere, the shortening of the spectrum being due to the gaseous constituents. Notwithstanding this, it was stated by Prof. Liveing, in a lecture delivered at the Royal Institution on March 9, 1883, that we must suppose the absorbent substance, whatever it may be, is not in our atmosphere, because, when observations are made upon the summit of an elevation like the Riffelberg, the lengthening of the spectrum reaches only a very trifle beyond U. "The same reason will lead us to reject the notion that the absorption can be due to matter in interplanetary space, for it is not easy to suppose that the gases which pervade that space in extreme tenuity can differ much from those in our atmosphere, because the earth in its annual course must pick up whatever they are, and they must then diffuse into our atmosphere. The absorption is, therefore, probably neither in our atmosphere nor in interplanetary space, and we must look for it in the solar atmosphere."

It is then suggested that the blotting out of the sun's light beyond U is caused by something in the solar atmosphere higher up than the metallic vapors which give rise to Fraunhofer's lines, but at a lower temperature. It has been shown by Young that the Fraunhofer lines are bright lines, but appear black against the brilliant light of the photosphere. It never appeared to me that Prof. Liveing's objection to Cornu's explanation of the limitation of the solar spectrum by our atmosphere was valid, because it was proved beyond question that the atmosphere absorbs the ultra-violet rays, and also that on the same day and hour, at different elevations, the extent of rays absorbed was proportional to the barometric pressure—that is, to the quantity of air through which the rays passed. A considerable acquaintance with absorption spectra in the ultra-violet region has proved to me that when an absorption band has been blotted out by increasing the proportion of substance, or by increase of the thickness of the absorbent layer, a stage is soon reached at which any further increase only causes a trifling difference in absorbent action, and in fact that many substances attain a maximum of absorption beyond which there is no change unless we increase the density of the substance, and so probably alter its molecular structure. Under the same conditions of pressure, increased thickness of the absorbent layer only very slightly increases the absorbent action, and that in a degree which is by no means proportional to the layer of material.

There is a difficulty in accepting Prof. Liveing's views, because we know nothing, as he remarks, in the solar atmosphere capable of causing such absorption and at the same time of transmitting the Fraunhofer lines of the less refrangible portions of the spectrum in the condition in which we observe them.

The matter was very fully considered by me two years previously—that is to say, in the year 1881. The absorption spectra of various gases were examined by photographing the ultra-violet rays which were transmitted by carefully measured quantities of gas at the atmospheric pressure, and one of these gases was ozone. It proved to be a substance with most extraordinary absorptive powers, so that even when very much diluted it exhibited an absorption band of great intensity, which was carefully investigated. By examining the spectrum transmitted by increasing quantities of ozone the band disappeared, and there was a complete and total absorption of rays extending to about wave length 3,160. Any further increase did not cause a corresponding shortening of the spectrum. The band was observed between wave lengths about 2,850 and 2,320; but with increased proportions of ozone the rays transmitted were restricted to about 2,920, and became more restricted in presence of greater quantities of gas.† Thus:

Extreme ray transmitted by ozonized oxygen, λ	Length of column of gas traversed by the rays, Centimeters.	Actual volume of ozone per square centimeter of sectional area of column. Cubic centimeters.
3,035	93	1·012
3,150	196·5	2·163
3,160	268·5	3·175

It was found that a quantity of ozone proportional to the average quantity present in a vertical column of the atmosphere caused an absorption similar to that observed in the solar spectrum—that is to say, terminating about 2,950. Largely increased quantities did not largely, but only in a trifling degree, increase the absorption. Furthermore, it was shown that the atmosphere contained ozone as a normal constituent, and that it was present in greater proportion in the upper regions than near the earth's surface. It was proved that all the other minute constituents of the atmosphere were either non-absorbent or exerted absorption in a manner different from that of ozone, and that the quantity of ozone commonly present in the atmosphere is quite sufficient to account for the limitation of the solar spectrum, without taking into account the possible absorption caused by the great thickness of oxygen and of nitrogen. The possibility of oxygen being the absorptive substance seemed very great, considering the small difference in constitution between the molecules of ozone and oxygen. It must be understood that the conclusions were arrived at by reasoning from strictly quantitative experiments, and seemed almost incontrovertible, and it may be stated that none of the facts alleged has ever been questioned. It was impossible to deal with the matter further without costly appliances for the compression of oxygen and nitrogen into tubes capable of holding a quantity comparable with the amount of oxygen and of nitrogen in a vertical column of the atmosphere, and for this reason the investigation fell into abeyance.

It was also considered that the problem might be attacked in another manner.

Messrs. Liveing and Dewar have recently made a very interesting and important communication to the *Chemical News* (vol. lviii., p. 163), on the absorption spectrum of oxygen. In a tube 1·6 meter in length, filled with the gas at a pressure of 160 atmospheres, all rays were absorbed beyond wave length 2,665, but they began to diminish at 2,705. With a tube 6 meters or 20 feet long, and with a pressure of 90 atmospheres, it seems that an absorption band is to be traced at wave length 3,640 to 3,600, and there is a complete absorption beyond 3,360.

The gas seen in quantity corresponding to that in a vertical column of the atmosphere appears to have a faint blue tint. There can be no doubt whatever that the oxygen of the air exerts a powerful absorption on the rays of the sun, but it does not appear from these experiments that this absorption is exactly the cause of the limitation of the spectrum, as described by Cornu, since when observed in tubes it is carried into a region of longer wave length than is observed at the level of the sea. Thus at Dublin the limit in summer is usually about 3130.* It is no doubt the high density of the gas which causes the absorption to be stronger than that of the atmosphere. It should be noted that the oxygen in the twenty-foot tube was the quantity in a vertical column of the atmosphere. It is probable that there are several substances in interplanetary space, or in the solar atmosphere, which, besides oxygen and ozone in the air, cause an absorption of the sun's rays and a limitation of the length of the spectrum, but, as Messrs. Liveing and Dewar point out, our atmosphere places a limit to the observations we can make on the rays of other heavenly bodies.

Touching the color of the sky, Prof. Tyndall has told us that four centuries ago it was believed that the floating particles in the atmosphere render it a turbid medium through which we look at the darkness of space. The blue color, according to his view, is supposed to be caused by reflection from minute particles, which can reflect chiefly the blue rays by reason of their small size. Experiments on highly attenuated vapors during condensation to cloudy matter were the basis of this reasoning. It always seemed to me that if a view be seen through a turbid medium which reflects chiefly the blue rays, it would not appear blue, but the complementary color, yellow; and, therefore, this theory could not account for the blue of distance. In fact, when a light mist hangs over the surface of the earth, and the rays of the sun are transmitted in a direction approaching the horizontal, the result is that the sun and all objects lying in the direction looking toward it appear yellow, while the mist in the opposite direction appears blue, and only translucent, not transparent. The blue of the sky, if caused by such a similar action of floating particles, would not be seen when the sun was overhead, nor could it be seen by looking in the direction of the sun.

The blue would not be transparent and in character similar to the blue of a clear distance, in which the outlines of mountains and rocks are perfectly distinct and sharp, the shadows being of an intensely deep blue, and the most distant objects the deepest in color. In 1880, Messrs. Hautefeuille and Chappuis liquefied ozone, and found that its color was indigo blue (*Comptes Rendus*, xcv., p. 523). On December 12, 1880, M. Chappuis presented the Academy of Sciences of Paris with a paper on the visible spectrum of ozone. He recognized the most easily visible of the absorption bands of ozone in the solar spectrum, and in consequence he stated that a theory of the blue color of the sky could not be established without taking into account the presence of ozone in the atmosphere, for the luminous rays which reach us will of necessity be colored blue by their transmission through the ozone contained in the atmosphere. And since ozone is an important constituent of the upper atmosphere, its blue color certainly plays an important part in the color of the sky. In March, 1881, quantitative experiments made by me were published to show how much of blueness could be communicated to layers of gas of different thicknesses when given volumes of ozone are present. I showed that ozone is a normal constituent in the upper atmosphere, that it is commonly present in fresh air, and I accounted for its abundance during the prevalence of westerly and southwesterly winds. It was likewise shown that it was impossible to pass rays of light through as much as five miles of air without the rays being colored sky-blue by the ozone commonly present, and that the blue of objects viewed on a clear day at greater distances up to thirty-five or fifty miles must be almost entirely the blueness of ozone in the air. The quantity of ozone giving a full sky-blue tint in a tube only two feet in length is two and one-half milligrammes in each square centimeter of sectional area of the tube. It is necessary to mention that a theory of the blue of the sky was propounded by M. Lallemand ("Sur la Polarisation et la Fluorescence de l'Atmosphere," *Comptes Rendus*, lxxv., p. 707, 1872), after his observations had been found inconsistent with all previous explanations. If the coloration be due to reflection from minute particles of floating matter, or if it be due to white light being transmitted through a blue gas, the blue portion of the sky should be polarized quite as much as white light coming from the same direction in the heavens. But the experiments of M. Lallemand prove that this is not so. Upon these experiments he bases his theory that the blue color of the atmosphere is due to a blue fluorescence like that seen in acid solutions of sulphate

* "Sur l'Absorption Atmospherique des Radiations ultra-violettes," *Journ. de Physique*, t. x., 1881.

† "On the Absorption Spectrum of Ozone," and "On the Absorption of Solar Rays by Atmospheric Ozone," *Journ. Chem. Soc.*, 1881, xxxix., pp. 57, 111, 119.

* In the *Phil. Mag.*, September, 1888, p. 286, Messrs. Liveing and Dewar refer only to my first paper on the absorption spectrum of ozone, *Journ. Chem. Soc.*, xxxix., p. 57, but not to the more complete paper on this spectrum at pp. 111–119, *loc. cit.*, which indicates the possible limits of the ozone in the atmosphere.

of quinine—that is to say, caused by a change of refrangibility in the ultra-violet rays.

Angstrom first threw out the idea of fluorescence being a property of certain gases in the atmosphere. To possess this property the gas must be capable of absorbing either in part or entirely the ultra-violet and violet rays, and of emitting them with a lowered refrangibility and without being polarized. Ozone possesses the property of absorption in the highest degree in the ultra-violet region, and I have now to announce that strongly ozonized oxygen is highly fluorescent when seen in a glass bottle two inches in diameter illuminated by an electric spark passing between cadmium electrodes. The color of the fluorescence is a beautiful steel blue. This fluorescence has not been observed in other gases, but it is in the highest degree probable that oxygen is fluorescent, though this has yet to be proved. There can be, however, little doubt that the color of the sky is caused in part by the fluorescence of ozone, and also to some extent by the transmission of rays through the blue gas. The blue of distance is doubtless to be attributed more to transmission than the blue of the sky, though it is quite conceivable that fluorescence also here comes into play. Whatever other cause concurs in the production of the blue of the heavens, it has certainly been established by M. Chappuis that the properties of ozone participate in its production.

In August, 1884, a very short note was sent by me to *Nature* concerning the red solar halo seen at Zermatt and on the Riffelberg with great distinctness. I recorded the occurrence of a dark band in the spectrum, slightly more refrangible than D, which was seen to vary in intensity; a second band a little less refrangible than D was also observed. On account of the altitude at which the observations were made, viz., 9,000 feet, and the state of the weather at the time, these bands were considered to be due to some constituent of dry air.

The subject of the telluric rays has become of increased interest since M. Cornu has studied the dark lines in the neighborhood of D, but unfortunately the rays absorbed to which I refer are both a little more and a little less refrangible than those figured on his map of this region. If we accept the number 5,890 tenth-metrets as approximately representing the mean value of the lines, D^1 D^2, the narrow bands observed by me have wave lengths about (1) 5,950 and (2) 5,770 at their darkest parts, as far as one can ascertain with a hand spectroscope giving excellent definition but small dispersion. They are very variable, being dependent on the state of the weather, and are more distinct and broader when viewed with the sun on the horizon. In London during the dry calm weather of June and July, 1884, they were very strong, but variable in different parts of the sky.

The less refrangible band, or broad line as it usually appears, below D, is generally overlapped by a band belonging to water vapor, the chief "rain band." On this account observations at an elevation of 10,000 feet or so during perfectly dry weather were considered of interest. The bands were observed against the blue sky on several occasions, but they were also at other times entirely absent or barely visible. There is some liability to a group of iron, barium, and other solar lines being mistaken for the more refrangible band when it is not decidedly strong. Chappuis observed bands in the blue sky coincident with ozone bands, and I have on that account always expected to find some indication of the spectrum of ozone in the upper atmosphere, but the reason why there must always be a difficulty in obtaining evidence of any absorption due to this substance arises from the strongest visible band of ozone, with wave length 6,095 to 5,935, being masked by the band of water vapor; and secondly, because the total amount of white light so preponderates as to overpower the effect of absorption—that is to say, the rays absorbed are only a small fraction of those transmitted, so that the bands are faint and the color due to absorption is either not seen or seen only with difficulty. Owing to this fact we cannot distinguish the blue color of the clouds when the sunlight is bright; but when the sky is completely overclouded with cumuli, a faintly bluish tint is given to the cloud shadows, even at the zenith. Near the horizon not only are the bright parts of the clouds blue, but their shadows have a rich blue tint.* The blueness varies somewhat; at times it may be seen to shift about in the sky; it has been observed, for instance, to pass over from southwest to northeast. The second but less conspicuous band of ozone absorbs rays with wave length 5,770 to 5,600. Both bands have been observed in a dry atmosphere at elevations varying from 6,000 to 10,000 feet, both in the blue of the sky and against white clouds. The measurements, very imperfectly made under difficulties, showed them to have wave lengths about (1) 5,950, (2) 5,770, in the center of the dark portion, while, according to Chappuis, the bands of ozone are:

(1) 6,095 to 5,935 mean, 6,010
(2) 5,770 to 5,600 mean, 5,680

On Angstrom's chart, a dark band, diminishing in depth toward the east, extends from 5,785 to 5,680, which is classed among the *raies atmospheriques;* this is similar to the band observed by me when viewing the sun or bright clouds near the horizon, and is similar to the second ozone band.

The work of Prof. Piazzi-Smyth, "Madeira Spectroscopic," does not give that portion of the spectrum which would serve for comparison. The "low sun band," δ, comes very near to band (2), wave length 5,770, while the "rain band" comes very near (1), 6,095. On several subsequent occasions the two bands were observed at lower levels than Zermatt and the Riffel, but with even less intensity. On the last occasion (November 10, 1884) that such an observation was recorded, there was only a trace of these faintly seen. No doubt a clear atmosphere, free from the turbidity so easily created by condensing moisture, is essential to their visibility. Schoene has observed bands in a bright sky before sunrise and after sunset, during an intense frost in Central Russia. The measurements taken identified them with ozone bands, and leave scarcely any doubt whatever of the presence of ozone in the atmosphere; and if it can be so recognized, it must communicate its characteristic blue color to the air

* Prof. Pickering has proved that sunlight as it reaches us is blue, which must be the case if it has passed through a blue medium (Proc. American Academy of Sciences, 1880, p. 236).

(*Journ. Chem. Soc. Abstracts*, vol. xlviii., part 2, p. 713). The remarkable crepuscular phenomena seen at the close of 1883 proved highly favorable to such investigations.

In order to continue a series of observations on the solar spectrum near D, it would be best to employ a fairly good dispersion and large lenses with long focus admitting a large amount of light to the eye, or, better still, to specially prepared photographic plates highly sensitive to the yellow rays.

The very extensive absorption of the ultra-violet rays by oxygen leads us to expect it to be fluorescent. All such absorbents are fluorescent more or less, and generally strongly, but when the absorbed rays are of very short wave length, the fluorescence is not always visible. Thus there are many substances which do not appear fluorescent by lime light nor by dull daylight, but are strongly so when seen by electric light, especially if it has passed through no glass or other medium than a quartz lens and a short column of air. Some substances are not fluorescent when seen in glass vessels, because the glass has absorbed those rays of which the refrangibility would have been lowered by the fluorescent substance. In air, and by the light of an electric spark rich in ultra-violet rays, such as that from cadmium electrodes, almost everything is fluorescent. The whole range of the cadmium spectrum has been viewed by me, owing to the fluorescence of the purest white blotting paper. The light, of course, is feeble, and the eye has to be trained to make observations in total darkness.

Pure water, however, never appears fluorescent. Some solutions in water, which transmit all the ultra violet rays as far as 2,304, are fluorescent, though whether this is caused by impurities or not has not been decided.

It cannot any longer be doubted (1) that the extreme limit of the solar spectrum observed by Cornu is caused by the gases in the atmosphere, probably both by oxygen and ozone; (2) that the blue of the sky is a phenomenon caused by the fluorescence of the gaseous constituents of the atmosphere, and probably ozone and oxygen are the chief fluorescent substances; (3) that ozone is generally present in the air in sufficient quantity to render its characteristic absorption spectrum visible, and that therefore it gives a blue color to the atmosphere by absorption, through which blue medium we observe distant views; (4) that water vapor does not participate in the coloration of the atmosphere under like conditions and in the same manner as ozone.

W. N. HARTLEY.

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A SELECT METHOD OF PREPARING NITROGEN GAS.

By H. N. WARREN, Research Analyst.

THE formation of nitrogen gas when required as an agent, either for the lecture table or when in larger quantities for research, always presents more or less

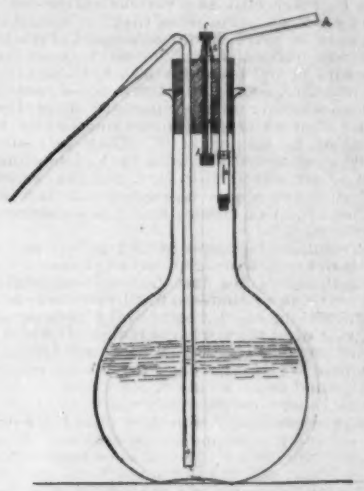
difficulty as regards its preparation; one process, still recorded in the various hand-books of science, being the action of nitric acid on raw meat. This method may, however, without doubt be regarded as nothing more than an alchemical memoir, since it would be evidently difficult to state, or enumerate, the complicated mixture of other gases which becoming evolved at the same time would naturally contaminate the product. The action of potassium nitrite when heated in a suitable apparatus in admixture with ammonium chloride produces, when prepared cautiously, a moderately pure gas, but owing to the inconstancy of the reaction the same is rendered considerably tedious and unprofitable. As a third instance, the reaction that phosphorus affords when burnt in a limited supply of air, as, for instance, when confined by the aid of a bell jar and in direct contact with water, at once presents an elegant and at the same time uncostly method of preparing a very pure gas, and direct from the atmosphere. This, although a favorite method of most demonstrators and others when experimenting with or illustrating the component parts of the atmosphere, yet at the same time few, if any, seem to have adopted a plan to recover, or more correctly to collect, the so prepared gas, which by a slight alteration in the form of apparatus to that generally employed may be procured in any desired quantity.

The accompanying figure, which is intended to illustrate the apparatus required, consists mainly of a doubly tubulated gas receiver or bell jar connected either with a bladder provided with a suitable stop cock, or, if required in larger quantities, to an ordinary gas holder, the outer vessel containing a sufficiency of water to allow of the gas jar at the termination of the action to be immersed to the required height. In using the apparatus, all that is necessary is to apply a hot wire to the pieces of phosphorus contained in a small brass dish, the same being allowed to float upon the surface of the water. This is most readily accomplished by withdrawing the stopper from the side tubule, inserting a hot wire in order to start the combustion of the phosphorus, and replacing the stopper. The apparatus and its contents, after the combustion of the phosphorus has terminated, is allowed to remain a few moments in contact with the water in order to allow the fumes of phosphoric pentoxide occasioned by the combustion of the phosphorus to unite with the water. The stop cock, in connection with the receiver intended for the reception of the gas, is now opened, the weight of the bell jar exerting a pressure sufficient to allow of its contents to be discharged into the receiver. The tap in connection with the receiver is next closed, the stopper from the side tubule again withdrawn, thus allowing, on raising the apparatus to the surface of the water, to admit of a fresh supply of air, which may be proceeded with as before. By this method, operating with a jar capable of retaining one cubic foot of atmosphere, a gas holder able to contain six cubic feet of nitrogen may be filled in less than an hour.—*Chem. News.*

AN AUTOMATIC WASH BOTTLE.

By J. F. JONES.

THE figure represents a section through the flask, and its fittings, which are just the same as those of an



ordinary wash bottle, with the exception of two valves. One of these is placed at the end of the air tube, *a*. It is a "Bunsen valve," which consists of a piece of rubber tubing, placed over the end of the glass tube. The lower end of the rubber tube is closed with a piece of glass rod. In this rubber tube there is a slit cut, which will allow air to pass into the flask, but will not allow it to pass out. The pressure of the air is thus increased in the flask, and an equilibrium can only be restored by water flowing out through the delivery tube. In order that this flow may be stopped when desired, the other valve is placed in. It consists of a glass tube, *c*, into which a small wooden rod is placed. At the lower end of the rod is a disk, which fits over the end of the glass tube, and to make it fit air tight, a rubber washer is placed at *d*. This disk is held tight against the tube by a small spiral spring, E. After blowing air in at A, and causing water to flow from the delivery tube, equilibrium can be at once restored, and the flow of water stopped, by simply pressing on the button, *f*. It is a very convenient piece of apparatus, as it saves the trouble of blowing during the whole time of washing a precipitate. By applying the mouth at A once, enough pressure can be obtained to keep the stream flowing for some time.

PRODUCING ALUMINUM AND OTHER METALS AND ALLOYS.

THIS invention relates to apparatus for producing, by electrolysis, aluminum and other metals from their refractory compounds. B is a series of carbon plates forming the positive pole, the negative pole being molten metal in a carbon crucible, A; *a* is an insulated box. The crucible is of carbon. Copper pins, a^1, conduct the current to interior of crucible, A. The positive electrode, B, dips into crucible, A. *g* is a clamp for holding the plates, *b*, and by means of which the electrode can be raised or lowered. The crucible, A, has a cover, *k*, of refractory material, in which are formed openings, *n*, for charging the material to be treated, which also serve as outlet passages for the gases produced. Plates, *o*, each having a handle, o^3, close the holes during the melting process. At the

Fig. 1.

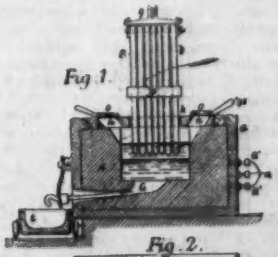
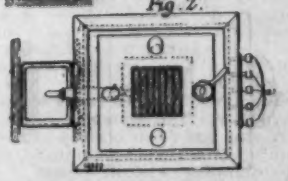


Fig. 2.



commencement of the operation the electrode, B, is brought lightly in contact with the granulated copper in crucible, A, a current is passed through and melts the copper which serves as the negative electrode, upon which clay or alumina is placed in the crucible and electrode, B, somewhat raised. The current will then pass through the alumina and melt and decompose it into its constituent elements, oxygen and aluminum.

The oxygen passes to the carbons, *b*, producing carbonic oxide gas, which escapes from crucible, and the aluminum passes to the copper and produces aluminum bronze, which may be run through tap hole, G, into an ingot mould, *t*, lined with carbon. The process is being introduced by the Schweizerische Metallurgische Gesellschaft, Neuhausen, Switzerland.

THE METEORIC THEORY.

IN a recent lecture at the South Kensington Museum, in aid of the Science and Art schools and museums, Professor J. Norman Lockyer said that the subject of which he was to treat had grown out of the work done in the science schools. He proposed, in the first instance, to give a general idea of what so far appeared to be the result of the work, running along certain lines of thought; then, to show how the new suggestions harmonized or clashed with received notions on the various topics raised; and, finally, to indicate specimens of the various modes of investigation, and the way in which the various phenomena had been allocated, together with the various inferences drawn. He would ask, to begin with, that, in imagination, a certain part of space should be cleared of matter, and certain possibilities should be set to work. Such a space would at first present a dark void, and the probability was that, sooner or later, in consequence of the conditions existing in other parts of space, that void would be filled with something so fine that no chemical name could be applied to it. Next, this something might be imagined to "curdle" into something more familiar to terrestrial chemistry, and the chances were that, if this hypothesis represented in any way the actual condition of things, that first substance would be hydrogen.

The "curdling" process would go on until there would be not only hydrogen, but an excess of hydrogen with an infinitely fine dust interspersed in it, which would continue to condense until there was a dust of that form which, when it entered the terrestrial atmosphere, produced the aurora borealis. It was in consequence of the messages which this dust, falling on the earth, conveyed that he was enabled to treat of the subject he had taken in hand.

Here and there in the space which was supposed to have been cleared there would be initial "curdlings," which need not be supposed as uniform. It was impossible, in the light of existing knowledge at any rate, to imagine that at any prior stage in the history of the heavens gravitation did not exist—that even the finest form of matter in the cleared space was not endowed with motion. Given the matter, the motion, and gravitation, and there would soon be formed centers. There would be a moment of momentum—a rotating motion of the "curdled" matter. The instant such centers were formed, gravitation would cause a motion of exterior particles toward the centers; and a condensation in one part of space would necessarily be counterbalanced by a clearing in another. So, if for one moment it were imagined that the "curdling" was not uniform, the condition of things would make uniformity less possible as time and the action went on. Let it be imagined that here and there were isolated eddies, and that here and there in the larger aggregations were smaller eddies.

The isolated eddies would appear by themselves, when their appearance was possible; but the smaller ones involved in the aggregates would be associated with the phenomena of the systems of which they formed a part. These cosmic molecules would be like the molecules of invisible gases, and there would apply to them the kinetic theory of gases in a very intensified degree.

The kinetic theory taught that with encounters or collisions there must be the production of heat, and from that the production of light. This light might be expected to be most delicate and to require very considerable optical power to discern it. Photographs of different *nebulæ* revealed what the eye never could and never would see; and, therefore, as the light was so dim, it was probable that the temperature was low. It was also probable that if this luminosity had been produced by collisions or encounters of the meteoric dust, the dust of a crushed meteorite, exposed to a low temperature, would present a similar spectrum to that of the *nebulæ* observed.

This had been done, and the spectra were identical line for line. If the *nebulæ* were condensations of meteoric dust, the condensation would proceed, encounters would increase in number, and the temperature would rise. This rise in temperature might result from three lines of action.

First. Having the rotation, it was obvious, by mechanical laws, that as condensation proceeded rotation would be accelerated, the collisions would be more violent, the temperature higher, and the light greater. There would be a central system with surroundings.

Second. Sir William Herschel had observed that it was a common thing for double *nebulæ* to be visible. If these were allied systems they must be in motion, and if a condition of things were imagined in which one swarm of dust were moving round another and a larger one, and occasionally intersecting it, there would be, when two swarms were nearest together, more encounters, and a rise of temperature of a periodic kind.

Third. There were these swarms of dust in space, and from the motions of stellar bodies they might be taken to be moving with enormous velocity. Suppose, then, that there were what might be called "level crossings" in space, where two or more swarms of meteorites met, moving in opposite directions or at an angle. There would be a tremendous cause of collision; and such instances were known in the heavens.

After the inrush of one swarm upon another there would be a sudden blaze of light. Now, there were cases recorded which could be easily explained on the meteoric hypothesis, but which were otherwise inexplicable. A star had been observed to suddenly rise from the ninth to the second magnitude, and almost as suddenly to diminish; and, in a part of the heavens where no star had been before observed, there had suddenly appeared a third magnitude star, which gradually died out. The eddies in globular systems would become brighter as their magnitude decreased; and so, from *nebulæ*, they would pass to what were known as stars; and one of the new ideas introduced was that a great many of the stars were not like the sun, but simply collections of meteoric dust, the particles of which might be from 30 to 50 miles apart.

The eddies of systems which were not shaple would vary in brightness, and in the case of the double *nebulæ* referred to there would be periodic variations of light, which explained simply a large class of variable stars.

Again, in each condensing system the condensation would go on and the temperature would rise until the radiation of heat equaled the increase of temperature due to the fall of meteorites. Provided that the supply of falling meteorites diminished or ceased, the temperature of the star would, by radiation, go on decreasing until some body like the earth was formed. Temperature could be divided into seven groups; and it was shown that there were seven well defined groups of bodies in space. Representatives of these groups having been found, about which there was no difficulty, all the celestial bodies could be placed in one group or another, and by means of the spectroscope the groups could be subdivided into species.

Those swarms of dust near which a system passed would be attracted to such system, and there would be a new order introduced, called comets. The tails of comets, it was observed, were produced in a direction contrary to that of the sun. If this could be proved of all comets, as it has been with one notable exception, and it were found possible to state the exact chemical conditions of comets' tails, it would be known what particular substances were repelled by solar energy. It looked very much as though they consisted of the permanent gases in meteorites. Thrown off as they were at high velocities and shining for millions of miles, it was not probable that the observer was dealing with the vapors of any terrestrial metals; but there was no reason why a permanent gas should not so remain for a considerable time. The exterior part of a system like our own, on the hypotheses adopted, would be vastly different from the interior part, and, therefore, the difference in density between the exterior and the interior planets might be explained.

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